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Portrait by Professor Olaf M. Brauner, 1917.

EDWARD BRADFORD TITCHENER

Edward Bradford Titchener

1867 - 1927

CO-FOUNDER AND CO-EDITOR OF
The Journal of General Psychology

This first number of *The Journal of General Psychology* was in press when the sad news came on the morning of August 3, 1927 of the death of Edward Bradford Titchener. The eminent Editorial Board and the general plan of this international journal of psychology have been formulated largely through his genius and energy. His death is a tragic blow to many plans which we had been making together. The publication of this journal will proceed along the lines already agreed upon in conference with him, and will be an international monument to his memory. In a later number of this journal will appear an article concerning him by a friend and fellow-psychologist.

CARL MURCHISON

October 1, 1927

THE HIGH RELIEF FINGER MAZE FOR HUMAN LEARNING.[1]

WALTER MILES

THE MAZE AS AN ENCLOSURE

From the Egyptian Labyrinth at Crocodilopolis to the amusing "Mouse-trap" in Chicago is a long and devious way in both time and space. Likewise, mankind's interest in mazes has been long continued and many sided. He has in fact ever been faced by just such spacial problems. The jungle and underbrush thicket, pathless or complicated by the winding and intersecting paths of cattle and other animals, the steep walled ravines, branching and tortuous, and the winding underground caverns, were all natural mazes and through practical necessity or curiosity man was prompted to solve them. Long ago in his planting and building he consciously imitated these confusing situations. The essential feature of the labyrinth or maze consisted in its being a complicated enclosure so arranged as to make ingress or egress difficult and success at first somewhat a matter of chance. It was of course assumed that an individual who undertook to traverse one of these maze enclosures, whether made of shrubbery or of pillars and walls, would use vision in a normal manner. But the hedges or walls are so much alike and dense enough not to be seen through, the alleys and openings are so nearly alike, and the turns and branchings of paths so numerous that vision can not perform its normal service of general orientation. The charm of one of these enclosures is just this oddity of visual experience: the individual can see where he is but he does not know where he is.

The application of the maze to the study of animal learning was a perfectly direct one. In the psychological laboratory exactly the same type of thing was made for the animal as had long been available for the entertainment of humans. These miniature enclosures were patterned after well known labyrinths as for example the

[1]Expenses in connection with developing the type of maze described were met from the Thomas Welton Stanford Fund for Psychological Research.

Hampton Court Maze which was planted about 1700. Thorndike (1) working with chicks in the laboratory of William James at Harvard about 1896-97 and later at Columbia University improvised simple mazes by placing together books or boxes (See his figures 13, 14 and 15.) Small (2) at Clark University working with Sanford a few months later corrected the Hampton Court pattern into rectangular form and copied it, making a maze specifically for work with rats. The total ground plan was 6x8 feet. It was constructed as one unit with a wooden bottom and with wire walls and top. The alleys were 4 inches wide and the walls were of the same height. So it was that the maze as a technique first came into the psychological laboratory and it was for work with animals.[2] New patterns were developed and novel maze features never tried in the labyrinths for man were productive of interesting results, and thus a new and important field of study was opened up.

Mazes large enough to accommodate human subjects make large demands for space and if they are built so as to really confuse visual impressions are quite expensive. Serious work with subjects enclosed within mazes began about 10 years after such work with animals. Hicks and Carr (3) did their study on a maze constructed out of doors. This they made by the use of posts on which strands of wire were fastened 2½ feet from the ground. The alleys were 2 feet wide, the total plan covered 14 x 20 feet, the true path was 78 feet in length and was complicated by nine culs-de-sac. The subjects, blindfolded, guided themselves by contact of the arms or hips with the single strand of wire on each side of the runways. Perrin (4) also working at Chicago and starting work in 1909 accomplished his work by securing the use of a maze in a Chicago amusement park. Practical difficulties in providing for and making such experiments on humans demanded a change in the maze equipment and the one most generally adopted was, I think, quite natural. The maze pattern in miniature as a set of slots (4, 5) in a metal plate, wooded panel, or some such material, was arranged to be placed before the blindfolded subject and explored by hand while he sat in one position in reference to it.[3] Here again we have the complicated enclosure and the sub-

[2]My colleague, Professor Calvin P. Stone, informs me that prior to this the maze situation, or something about like it, had been used by Sir John Lubbock for studying ants.

[3]Slot or stylus mazes that will permit of the use of vision have also been designed. See references 6 and 7.

ject trying to get out, while actually he isn't in, for the "tracing" is usually done with a pencil or stylus.[4]

The animal or human walking down an alley can easily notice when an opening on either side is reached and so can decide whether or not to enter. "Vision provides a comprehensive, ultra-rapid, spatial survey" (7) and he covers the entire cross section as he goes. With stylus or pencil tracing, so called "extended touch", the subject must as a rule press on one side or the other.[5] He has no good way of comprehending the full cross section of the path as he proceeds, only vision or direct cutaneous contact can confer this information. This chance factor of pressing on the right or wrong side is an important element in stylus maze learning. It seemed to the writer that if it could be avoided by doing the tracing with the finger tip on a very narrow path the results might conceivably be quite different and rather more comparable with those gotten from humans or animals exploring within maze enclosures.

The Maze As A Flat Design

Somewhat earlier than the introduction of stylus mazes which are really of the enclosure type we find publications referring to work done with paper mazes. These designs represent the "alleys" as bounded by parallel lines and are really drawings of the hedge mazes. They are of course only applicable to human subjects and as ordinarily made absolutely require the use of vision. Inlay mazes made of different materials and to be followed by touch have not, so far as I know, been used. Paper mazes have served as tests of motor control rather than of learning. Subjects were required to trace the paths endeavoring to avoid contact with the

[4]Pencil tracing, of course, has the advantage that a record can be inscribed on paper under the maze (4). Perhaps this is the reason why the stylus has come to be used rather than the finger. The grooves could be traversed by the finger altho the slots would have to be made in such a way that the finger tip would not be injured when moving rapidly on them. Max Meyer, A Brief Manual Of Psychology Demonstrations, Columbia, Missouri, 1922, see pages 15-16, recommends the use of the finger. His mazes, as metal castings with simple half-round grooves rather than slots, are made by C. H. Stoelting, Chicago. As supplied by that concern they are rather rough for the finger but can be much improved by coating them with automobile lacquer.

[5]The method of continuously touching one side, while of course keeping the eyes open, has been a favorite way of traversing complicated hedge mazes. It will lead the wanderer in and out of several blinds but as a rule finally to the center. The so called key to the original Hampton Court Maze (8) is to "keep the right hand continuously in contact with the hedge from first to last, going around all the stops." The method will not work for the left hand in this maze.

boundary lines. Whitley (9) in 1911 described designs for this purpose. Some were constructed all of curved paths and others of regular right angle turns. In the same year Kent (10) described work with dementia precox patients and illustrated a rather complicated paper maze out of which subjects had to trace with a blunt point "without crossing a line and without raising the marker." The Porteus (11) paper mazes to be traced by pencil are well known as motor-intellectual tests. In giving some alcohol results on eye and hand coordination Dunlap (12) illustrates a test blank which is really a maze. It is like one of the Whitley tests (9), a regular right angle turn pattern, but Dunlap has introduced a number of adjoining lines which are to confuse vision and make the directing of the pencil difficult.

Mazes in the form of a broken spider web or some other much complicated design are frequently published in magazines and newspapers. They are a recognized form of puzzle that can be solved by the eye with perhaps at times some help from the hand in keeping the place while the eye explores a complicated cul-de-sac. The eye must follow these paths after the same manner as in reading a line of print "with hitches and starts" that is, by use of the saccadic movements and momentary fixations. This demands careful motor control and often retracing, or in terms of reading, "regressive movements". It is interesting to notice that these puzzles are

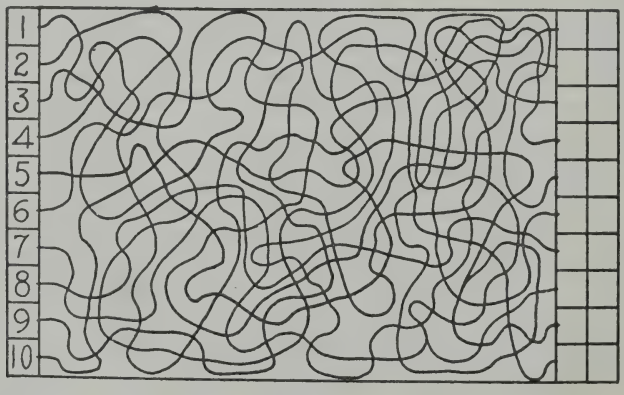

FIGURE 1
THE MACQUARRIE EYE-MAZE

usually diagrams of inclosed paths. MacQuarrie (13) provides us with a deviation from this and gives us really an eye maze where

the paths to be followed are single lines. This he developed in the Psychology Laboratory at Stanford as one of his tests of motor-mechanical ability. He calls it "pursuit". It was suggested by the pursuit problems with which we had been working. One of his practice designs is here reproduced full size as Figure 1. Starting with one of the digits on the left the eye takes the line connecting with that square and without finger pointing follows it through to discover where it ends on the right. The subject then numbers that square accordingly: More than one line may end at a certain place on the right, hence the double column.

There is a recent important development in this second fundamental type of maze, that is, the flat maze, which merits special comment. Perkins (14) working at Michigan with Professor Shepard has developed a way of using flat mazes outlined on paper or tracing cloth and of securing typical learning through visual experience. These mazes on a glass table top illuminated from below are explored visually through a tube 10 inches long and 1 inch in diameter. The alley width employed was ¾ inch. The tube rests on the glass and its upper end is surrounded by a cardboard screen 12 inches square which is very near the subject's face. In this way, at any one moment, the subject can see only a round area 1 inch in diameter which is just a little more than the width of one alley. The tube is moved by hand as the subject proceeds. He may go in any direction so long as he does not cross a line. The maze is placed under glass so that it will not accumulate the marks of wear. This scheme has the important advantage of more nearly resembling the visual experience of being in the maze, seeing clearly a small part, but not being able to use vision for complete orientation. However, as the author points out, her flat "maze is not comparable with those used for animals, where the animal is put into the maze and runs it with his whole body."

The Maze As An Abstraction,—Mental Maze

What seems to the writer the third fundamental type of maze that is recognized in the experimental literature may simply be mentioned in passing. This is the "mental maze" or the maze situation carried into the choice of alternatives which may be given as auditory stimuli. Peterson (15, 16) has fully described this important development. According to the instructions which he prints for the subject, he causes the subject to think of a maze rather than to see or to touch one. No two parts in the maze are lettered the same. Two letters are called at a time and the subject chooses one of them. Which letter is called first is a matter

of chance and the position of a letter in the maze has no connection with its place in the alphabet. The problem of course is to reach the goal with the fewest errors. In the second publication he recommends that when the subject makes a wrong choice he be immediately started back at the very first and given the alternative choices at the first blind. This is a unique feature. As Professor Peterson states, "Of the various maze situations that have been presented either to animals or to man, such a procedure is of course possible only with the mental maze."

The Maze As A Raised Outline Or Path

The occasion for this paper is to call attention to what seems to be a fourth fundamental type of maze which is applicable to at least some animals and for human subjects appears to have several natural advantages. From the foregoing discussion it is perhaps reasonably clear why the maze is commonly thought of as a complicated enclosure. But there is no essential reason why the concept need be limited to this. It may as well be a set of *elevated paths* to be followed and this form has its prototype in the interlacing limbs of the trees which the squirrel follows or in the situation of a man picking his way along on the stones in the shallow water of a river bed. Some animals seem to prefer running on a fence or a log to going on the ground or below the surface. The rat seems to have about equal facility. He can run along on a rope with ease and at our ports we have to provide against his landing by this means. In November 1925 the writer built and began using an elevated skeleton maze with rats (17). The width of path was 1 inch. The path has no sides and is 30 inches above the floor. It is made in units 36 inches long, rather like race-track hurdles. These are set touching each other making a pattern of Ts and Ls so that the animal may make its way on the top from one to the other. The cul-de-sac in this case is a dead end or jump-off.

An extension of this idea of making the maze path about as narrow as possible and of building it as a raised path rather than as a depression has taken shape in the high-relief finger maze an example of which is shown in Figure 2. After quite a little preliminary experimentaton it was found that very practical and at the same time inexpensive mazes could be made by using wire on thin wooden panels. German-Silver wire in size No. 14 (1/16 inch diameter) has been found quite suitable. The wire is bent into long square cornered staples of suitable lengths for the pattern desired and these are driven into the wooden panel. Thus we pro-

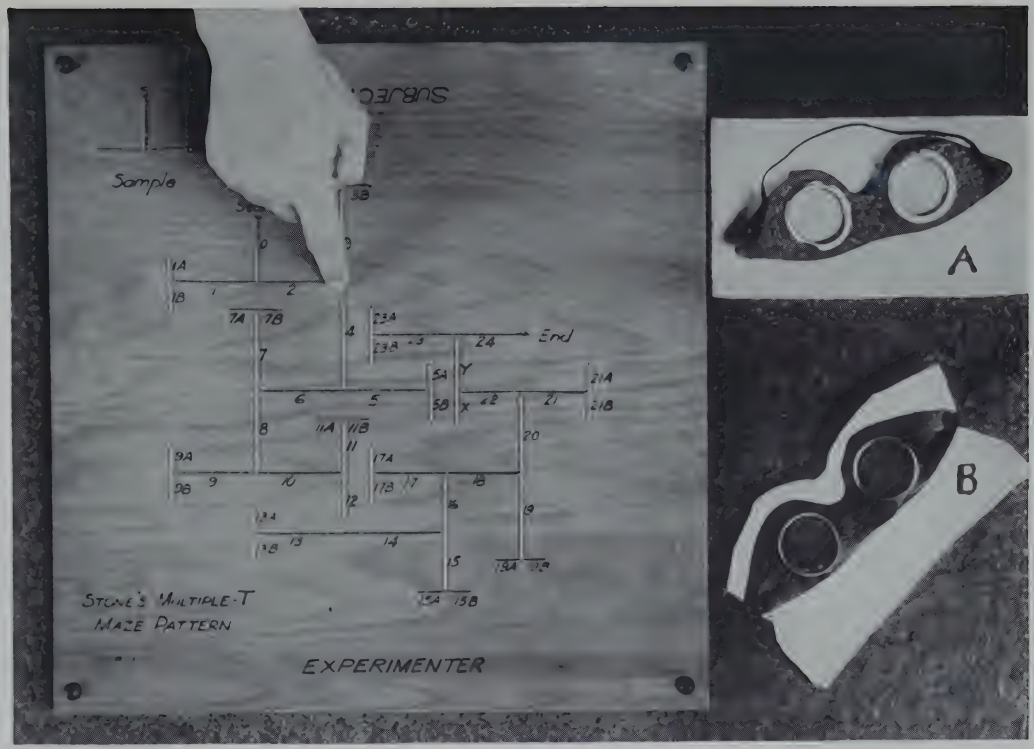

FIGURE 2

THE HIGH RELIEF FINGER MAZE MADE OF WIRE STAPLES DRIVEN INTO A CEDAR PANEL. A AND B SHOW STANDARD BLINDFOLD EQUIPMENT

vide a rigid, permanent outline of the maze pattern raised from the surface of the panel by the diameter of the wire. We have generally used 3-ply panels of white cedar 18 inches square and have made the patterns so as to cover about 12 inches square. It is practical to place a pattern on each side of a panel and by tacking the ordinary rubber feet at the corners to arrange to protect the desk or table on which the maze rests. The rubber feet serve to make the panel more stationary while in use.[6]

The raised maze is to be traced by the finger as shown in the illustration. A bit of "sample" path is provided so that this may be explored to acquaint the subject with the nature of the structure, before he actually begins on the design that is to be learned. To simplify and regulate the recording of results we have adopted the convention of numbering the parts. The blinds are given odd numbers and the parts if more than one are lettered. The start position is indicated by a small staple driven astride the path at one end. The goal or "end" is indicated by two such staples. This feature is also included in the "sample" section. In the maze illustrated the blinds simply come to dead ends where the subject's finger will slip off the wire. In some other mazes we have placed an obstruction in the form of a small staple at the end of each blind. In such cases it is of course necessary to use a different kind of indication for the start and finish.

Certain considerations seem to have a bearing on the proper unit dimensions for such a finger maze. The distance between turns or choices might be so small as to introduce confusion or again too large for any use. In working over this question of the optimum size for spacial units in such a pattern we note the following. The average length from the tip of the index finger to the elbow for the adult is about 40 cm. A circumference with this as a radius gives 7 mm. as 1 degree of movement arc. Using the familiar value for least perceptible passive movement at the elbow as 0.5° at the rate of 1° per second, and for the shoulder of 0.3° at the rate of 0.6° per second we see that a 40 mm. cul-de-sac is probably

[6]Our first raised finger maze was made from narrow oak strips glued to a panel. It is difficult to have the strips sufficiently narrow so that subjects will find all the leads. The wire can be used as loose staples placed in holes drilled in a regular manner so that patterns may be formed as desired. Straight or curved pieces of wire can of course be used. The wire (not staple form) may be soldered lightly to screws or tacks inserted in the board. Other materials than wire suggest themselves and have various advantages. Mr. Siegen K. Chou at our laboratory has produced some interesting maze paths by using shot, round headed tacks, etc. in boards and metal screen.

somewhat longer than necessary. It would amount to around 4 times the diameter of contact area of the finger tip and to near 6° on the arc of rotation at the elbow, and to more than 10 times the average least perceptible difference. As a general principle we have adopted 35 mm. as a satisfactory dimension to be used between turns or choices. This, at least, is not too small and no one has complained of its being too large. It is the turns chiefly that give character to the maze tracing experience and these should not be too far separated.

The shape of the maze and of the blinds is a matter for special investigation and for decision in reference to a particular problem. An analysis of maze joints and cul-de-sac shapes has been made at Stanford by Mr. I. Huang (18). The high relief finger mazes have a greater range of possibility as to shape and also complication of blinds than have the slot mazes as they have no tendency to fall apart by having certain sections cut free from any anchorage. The wire mazes lend themselves very readily to making changes in any pattern; inserting or pulling out a piece is a matter of a moment's work.

As for the results with the raised finger maze some data and discussion are given in the accompanying article by Husband (19) and other material will be published in further articles. In general it appears that learning is much faciltated by using a maze pattern in this finger maze form. Judging from the data given by Husband, having direct cutaneous contact with the narrow wire path as against using a stylus, "extended touch" (Warden data), in a slot maze of the same size and shape, reduced the mean number of trials to learn from about 70 to 17. We do not yet know whether this is a typical result for this maze type comparison and so must suspend judgment until work in progress by others is ready. It seems fair to conclude, however, that having the direct finger contact experience confers a great advantage and saves the whole maze performance from the gross chance error of happening to press the pencil on the wrong side of the path.

A Standard Blindfold Technique

To allow the subject who is to be tested on the maze to have his eyes open in an ordinarily lighted room while the maze is covered with a canopy or is hidden by a screening booth is a routine often used. Considered strictly this is hardly a non-visual condition. Aside from the chance that the subject might sometime get a glimpse when he inserts or withdraws his arm from the curtain (and subjects do zealously hope for such a glimpse) it is surely

possible as he moves about during the tracing that he builds up an elaborate visual scheme from the objective surfaces before him. No data is at hand to prove the point but it seems likely that this routine is subjectively quite different from working in complete darkness or when wearing a really effective blindfold. Probably the conditions can be more uniformly controlled if a thoroughly comfortable blindfold is employed. We have found a readily available, simple and inexpensive means of satisfying this blindfold condition and wish to recommend it for adoption with human subjects in any maze work where vision is supposed to be ruled out. In Figure 2, A shows the items to be used and B illustrates them in position having been just removed from a subject. The glasses in a cheap pair of leather automobile goggles are covered by surgeons adhesive applied to the inside. A double thickness of thin crape tissue paper, such as is commonly used to remove cold cream or similar substances from the skin, is placed over the goggles so that when they are brought up to the face the tissue will be between the goggles and the face. The tissue fulfills the following purposes:

(a) it completes the blindfold as it extends beyond the border of the leather and it fills the spaces between the leather and the face, (b) it is soft and comfortable, (c) it absorbs the moisture on the surface, and (d) by using fresh tissue for each subject the goggles remain sanitary. This blindfold may be easily arranged to be thoroughly efficient and at the same time very comfortable for a long session of work. The subject can keep his eyes open, he has the stimulating effect of light, he may be able to count his fingers held near his face as he stands by a window, but he cannot possibly see any more detail than that.

Some further general suggestions for procedure with the finger maze experiments are as follows:

1. Have the maze covered with thick paper held in place by an elastic band. Always keep it thus covered when not in use.
2. Instruct the subject that he is going to learn the form of a raised pattern by passing his finger over it while his eyes are covered with a comfortable blindfold.
3. Seat the subject at one side of a rather narrow table and seat yourself opposite him. Have chair and table of usual height, that is, 30 inches for the table.
4. Place the covered maze squarely and directly in front of the subject while his eyes are open. Have it properly orientated with the elastic band removed so that when ready the cover may be turned back without moving the maze.

5. Apply the blindfold to the subject and make sure that it is efficient and comfortable. Tell him he should keep his eyes open behind the blindfold.

6. Uncover the maze and have the subject rest his fingers lightly at and on the lower corners or the panel but not touching the maze pattern. The hand that is not used in tracing should always be resting at its corner. This constant orientation for the unused hand is an important condition.

7. Take the index finger of the writing hand and place on the "sample." Then instruct the subject about the nature of blinds, goal position, and the like. The finger should be applied to the maze at an angle of about 45 degrees and the other fingers should be flexed as when pointing with the index finger.

8. In the trials the experimenter writes down the numbers of the blinds entered using care that the noise of writing is not a distraction.

9. Start the watch *when* the subject starts.

10. Have the subject place his tracing hand in his lap when he has finished a trial.

11. Give no suggestions while he is at work making choices.

12. If it is desirable to control the element of retracing, instruct the subject that he will be told each time he retraces and that it will be counted one error. Tell him just as he reaches the position where he may go into the next blind back. It is probably a good plan to actually block him off from going farther back by holding a pencil rubber or your finger on the spot.

Summary

1. The characteristics of the four fundamental types of mazes for use with animals or humans or both are discussed and compared. These four types are (a) enclosure mazes, composed of labyrinths, hedge paths, reflection corridors, and slot or groove mazes to be traced with pencil or stylus, (b) flat diagram mazes, for human subjects only and requiring vision; these are essentially eye mazes, (c) mental mazes such as devised by Peterson; the subject makes a choice between two, or more, alternatives without seeing or touching a diagram, and (d) high relief or elevated mazes such as developed by the author for use with rats or as finger mazes for human subjects.

2. The article is chiefly concerned with this last type, that is, raised mazes, and describes and illustrates finger mazes made of wire applied to wooden panels so that the maze may be traced by passing the finger along on top of the designs.

3. Advantages of the high relief finger type over the stylus maze of similar size and pattern are: (a) the subject has direct cutaneous contact of spacial area rather than a mere point of extended touch, (b) the subject encompases the whole width of the path and does not have the experience of pressing on the wrong side as with the pencil or stylus in a groove, (c) the maze can be traversed rather more rapidly (d) raised wire mazes are much less expensive to make than slot mazes of metal, they can be easily duplicated for elementary classes in laboratory and educational psychology, can be easily modified and can be more complex than the slot type.

4. Results thus far secured show that learning is greatly facilitated in the finger maze as compared to the same pattern and size as a stylus maze. The number of trials to learn is apparently reduced to one half or one third. Therefore the finger maze is more available for work with children.

5. Blindfolding the subject is strongly recommended as a procedure for maze work and a standard blindfold routine is described in detail with other suggestions for carrying out finger maze experiments.

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HUMAN LEARNING ON A FOUR-SECTION, ELEVATED, FINGER MAZE* [1]

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INTRODUCTION

The maze situation as a means of studying learning needs little introduction. Some of the advantages of using it are: (1) Its objectivity as applied to scoring; (2) Flexibility in varying the number of turns, complexity of path and of possible errors; (3) The successful act is not achieved all at once, as in a puzzle box solution; (4) The same type of apparatus can be used with adults, children, blind, and animals; (5) There is little physical demand or fatigue produced in the subject; (6) It is a miniature performance, dispensing with elaborate apparatus; (7) The ordinary person is easily interested in the task and tries hard to succeed in it.

The maze employed in this investigation had three unique features. First, it was a high relief type of finger maze, devised by Professor Miles of Stanford (1). This is wholly different from the usual slot maze in which a pencil or stylus is used in tracing the pattern. There is no chance to hug the groove wall on one side or the other, and thus pass by an adjoining pathway without noticing it. Second, with the finger tip in direct contact with the pattern the subject secures important cutaneous experience in addition to the usual kinaesthetic cues. Third, it was made up of four sections so that the learning of each could be examined separately. It was of the multiple-T construction, of the Warden design. Other mazes have been made in the multiple-T style, but the choices have been in all directions, which introduces extra complicating factors in the results. In this style, however, every choice is in one axis in reference to the subject, either right or left.

STATEMENT OF PROBLEM

The purposes of this study were the following: 1. To ascertain the order of learning thru a study of the order of elimination of errors. The use of a complex maze with four logically separable but continuous sections permits comparable treatment of the results for the various sections and for the individual choices within each section. 2. To study the learning process in a very complex

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maze, in this case composed of forty choices divided into four sections each with ten choices. 3. To contrast the factors in learning a one section maze as against a four section maze of similar patterns. Are four times as many trials required to learn the four sections as for a single section? 4. To study the causes of individual errors; why some are made more frequently and are harder to eliminate than others. Are all subjects bothered by the same errors, or are there wide individual variations?

Apparatus and Method

As mentioned, the maze was of the multiple-T variety, constructed after the pattern described by Warden (3). His pattern was copied here for the single section used in the preliminary experiment and was used also as the first section in the four part maze, in order that our comparisons between the stylus and finger types might be the more valid. The other three sections were made up of similar series of choices, of varying theoretical difficulties. Section I had six changes of direction, II had eight, III had five, and IV had five. This is brought about by placing in various positions within the sections sequences of two and three choices in one direction, interspersed by single choices to right or left.

The sections were very distinct from one another, being connected by elevated pathway links several times the length of an individual alley. Also, as may be seen from the diagram, there is a change in general direction after each section; the first and third proceed away from the subject; the second and fourth come back toward him. The subject started at the lower left hand corner and finished at the lower right hand. This breaking up of the total into distinct sections gives logical divisions for purposes of treatment of results and also to the subject in learning the maze. The Warden pattern lends itself ideally to this purpose.

The subjects were largely psychology students, some being departmental majors and some enrolled in elementary courses. A total of 80 in all were used:—20 learned the single section, as a control group; 40 learned the four-section pattern in the order indicated on the diagram; 20 more learned the forty blind maze with the sections in changed order, namely, III, IV, I, II.

Before starting, the subject was given a brief explanation of the nature of mazes, and the general T-type of construction was mentioned. To demonstrate this, after the blindfold was in place he was allowed to feel over a small sample section of two blinds which was separate from the rest of the pattern. The blindfold

was a pair of ordinary goggles with adhesive tape over the lenses and a sheet of tissue between the goggles and the face, the standard blindfold recommended by Miles (1). Then after being told not to hurry, as the emphasis in the results was to be laid on accuracy rather than speed, the experimenter placed the subject's finger at the starting position.

Record was kept of time, trials, and errors. If a subject started up an alley, but corrected himself before coming to the end, it was not scored as an error, as this change was really to his credit rather than against him even if it was a deviation from the true path.

Trials followed one another as fast as convenient for the subject, and for the experimenter to jot down the results of the previous trial.

The criterion demanded for learning was three perfect trials in a row, or three perfect out of four attempts, to allow for a chance error. When the criterion had been met and the learning was thus considered complete, the subject was questioned in regard to his method of learning.

Results: One Section Maze

1. *Trials to Learn.* The average number of trials for the twenty subjects learning this single section maze was 16.7, standard deviation 7.5, range from 5 to 31, including the three perfect runs. This contrasts in a very surprising manner with the results obtained by Warden (4). His subjects took an average of 69.4 trials to learn the same pattern, yet the only apparent difference was that he used the stylus type, while we used the elevated finger maze. Further, his best subject required 16 trials to learn, which is only a fraction of a trial less than our average for the twenty subjects. Possible explanations are: 1—In the finger maze one can not lean to one side or another, as is necessary in the stylus type, thus passing by alleys, true or false, without realizing it. 2—There is a more personal contact afforded by the finger touching the raised pathways than the indirect contact of the pencil in the usual stylus type of maze. 3—This leads to the third reason, found in analysis of the results, that the time of each trial was so much cut down that a well-integrated pattern could be built up.

2. *Learning Curve.* A very typical learning curve, which is omitted here, is obtained by plotting the number of errors on each trial. In making all curves it was assumed that a person who had satisfied the criterion of learning would continue to make errorless trials, so the total number of errors was always divided by the

total of subjects who started, regardless of whether all continued to the trial in question or not. On the first trial there is almost exactly a chance number of errors, there being 99 where 100 might be expected, as there are twenty subjects taking ten even chances each. The curve drops fairly rapidly thru the first four trials, but from there on the drop is less pronounced. After the fifteenth there is less than one error per subject, so it is practically negligible.

3. *Methods of Learning.* Warden (4) classified his subjects according to modes of attack as verbal, visual, and motor. None of our twenty subjects learned by forming a visual pattern, so this classification is impossible here. Moreover, there is an additional class; this is learning the maze partly in a motor manner, but finishing by counting the turns (verbal). This should not be classed as verbal, as a few trials were wasted in a motor manner, yet some of the pattern was learned during this time. Table 1 shows the relative efficiencies of the various methods.

TABLE 1

METHODS OF LEARNING FINGER MAZE

Method	Number Using	Trials
Verbal	9	10.7
Verbal; Motor Discarded	5	17.2
Motor	6	23.0

The proportions between the first and third groups are somewhat similar to Warden's findings. His subjects using a verbal method took 32.2 trials, while those relying on a motor method required an average of 123.9.

4. *Individual Errors.* Errors made in each blind alley in this section, which has the same pattern as section I of the four section maze shown in the chart, are shown in Table 2.

TABLE 2

ERRORS ON SINGLE SECTION MAZE

Alley	1	3	5	7	9	11	13	15	17	19
Errors	53	25	105	165	27	83	85	46	148	38

By comparing these figures with the diagram of the pattern we may see which alleys presented the most difficulties to the subjects. As in the experiment with the four part maze we see that errors increase progressively thruout a sequence of like choices, both of threes and twos. The fewest errors are made on alleys which break a sequence. There is little relation between initial errors and totals made thruout the whole of the learning, the correlation

being + .20. This is rather to be expected, as all alleys are of the same shape, and there is little or no difference in position making for likelihood of entrance into one alley over another, as there is in more irregularly designed mazes. Comparing these results with Warden's the proportions are very similar, showing that apart from difficulty in learning, about the same factors operate in the stylus and finger mazes.

5. *Comparison with the Same Section while in the Four Section Maze.* When alone the single section is learned in 16.7 trials; when first of four similar sections in 23.2 of an average of 29.0 for the whole maze; when third of four it is learned in 20.4 with an average of 22.3 for the whole maze. From this we see that the presence of other sections is a distracting influence. Second, that it is learned earlier in the whole learning process when located on the outside of the whole pattern to be learned. Third, comparing the errors we find a total of 775 when alone, 1082 and 1094 when first and third respectively. The proportions are very largely the same under all circumstances, the chief difference being that the final error is made with much more frequency when another section follows. The first choice in this section is also made incorrectly many more times when it is in an intermediate position than when first in the whole maze. Otherwise there is close resemblance. The correlations of errors are + .43 between the single section and the same when first; + .66 between the single section and the same when third; + .76 between the section when first and when third.

Results: Four Section Maze

1. *Trials to Learn.* The average number of trials was 26.8, standard deviation 13.6, range 6 to 69. This average was far below expectation, especially considering that the average for the single section was 16.7, which in turn was thought to be very low in the light of Warden's results. It was expected that four equal sections would more than quadruple the difficulty in learning, since the four were done one immediately after the other, thus distributing the subject's attention more widely, interfering with memorization and reducing transfer to a minimum. However, we can raise this difference somewhat by eliminating the three perfect trials, which do not contribute to the learning but come after it, and possibly the first three, which are more or less preliminary orientation. This makes the totals 10.7 and 20.8 respectively, but this is still far from showing four times the difficulty of one section. So

it would appear that there is more difficulty involved in making a proper approach to the problem than in the actual mechanical memorizing after a method is devised. Another possible explanation is that the subject is forced by realizing the complexity of the larger pattern to devise a better and more efficient mode of attack than is necessary in learning the single section.

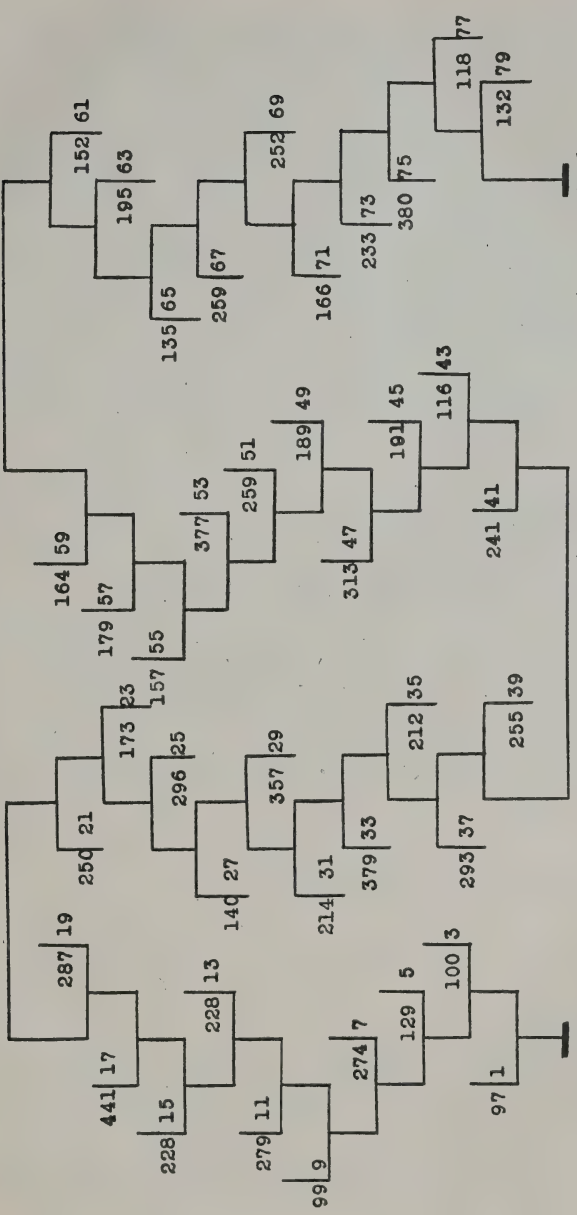
2. *Curve of Learning.* The curve is very constant in its decrease, there being no prominent steeples and only a few minor fluctuations. This would indicate that the learning was very regular, but this is partly due to the balancing of the averages by all the subjects; the impression gathered from watching the sixty subjects who learned the four section maze was that the learning was more in jumps with decreases of four errors or so after a plateau during a few trials. The number of initial errors is always below chance, on the whole maze and each individual section, indicating some reasoning was present even on the first trial. Often more errors were made on the second than on the first, showing that the slight amount of memory conflicted with reasoning with detrimental effects. Curves for the individual sections show the same characteristics as that for the whole maze, with a few differences due to varying amounts of difficulty in learning each section, which will be pointed out later.

In ten instances the learning period was broken into two parts, with intervals varying from three to forty-eight hours. In number of trials no comparison is possible, as all subjects who broke up their learning period were over the average, as otherwise there would have been no occasion for interruption. The number of errors before and after the interval however may be compared. In the two trials just preceding the interruption there were 87 and 84 errors totally, while in the first two after the learning was resumed the errors were 97 and 78. This indicates that there is only a very slight loss during the interim, causing just one poor trial. Two subjects who took only a few hours' time out showed marked improvement immediately after coming back to the task. This might be taken as in favor of Watson's theory of frequency; the correct alley was traversed every time, and so died down less than conflicting impulses with recency value during the rest period.

3. *Individual Errors.* The total number of errors made on each blind is given on the chart for purposes of better comparison than a table would afford, as the actual situation is portrayed.

Effect of Sequences. The outstanding fact to be noticed is that, with a single exception, the errors increase progressively from one

FIGURE 1
PATTERN OF FOUR-SECTION MAZE



The Blinds are Numbered Consecutively from the Lower Left (Start) to the Lower Right (Goal), and the Total Errors made by 40 Subjects are Entered for each Blind.

member of a sequence to the next. For example, alleys 3, 5, and 7 are all left turns, the errors made are respectively 100, 129, and 274. This is true in sequences of both two and three thruout the maze. The single exception comes at the end of Section III. Possible explanation may be that being at the end of a section this sequence is learned more quickly than if it were in the middle. At the end of Section IV there are two lefts, but we would expect to find very few errors on the first of the two, as it makes a break following three in a row in one direction, so the slight increase (from 118 to 132) very likely indicates that there would be a decrease under other conditions. The general trend of errors, however, shows an apparent tendency to alternate. This has also been noticed on true false examinations; where there are three correct (or incorrect) consecutively there are more mistakes made on the second than on the first, and still more on the third. Keeping near the median line of the maze may be a further factor.

Initial Errors. The errors made on the first trial correlate + .41 with the total made during the whole learning, which shows some tendency but is not particularly valuable, especially since an inspection of initial errors shows that the range is very much less than in the totals for the whole learning period.

Persistent Errors. Each subject was bothered more by one or more alleys than by others. In order to see if there was any uniformity between subjects the most frequently made error was found for each subject. Of the alleys thus named, the four most troublesome for individual subjects were all within the five most frequently made in the whole maze, showing a good uniformity. Three of the four most frequently made errors were third members of sequences. The last errors to be eliminated are also very like the ones most frequently made. The two of course are more or less dependent on one another.

Comparison of Most and Least Frequent Errors. Alley 17 was the most frequently entered alley, 1 the least. As compared with the curve for the whole maze, 17 is considerably above the average up to the thirtieth trial, which was as far as the curve was plotted. The difference did not manifest itself until about the sixth trial. Number 1 on the other hand was eliminated very early, first meeting the abscissa on the twelfth trial, altho afterward there are occasional errors.

Corrections were recorded as such when a subject changed his direction before coming to the end of an alley. Those ending cor-

rectly outnumber the ones resulting in errors by about a three to one ratio. Often a persistent error is broken up with a few corrections interspersed among the correct choices and errors, a process analogous to the shortening up of entrance in animals.

Difficulties were encountered from a few other causes. One very frequent cause was in taking a sequence before its proper place; this is taken to explain the frequent errors on alley 47. Analysis of the pattern shows that this blind follows a sequence of two lefts; immediately after it there are three lefts; the three tend to be taken where there are only two. Another cause of error came from the tendency to alternate; if a person made a right hand choice on one alley and went wrong, he often would choose the left on the next, apparently compensating. For this second choice to be right there would have to be two in a row in the same direction, which was not always the case.

4. *Correlation Between Trials to Learn and Errors.* The correlation is $+ .84 \pm .02$, which shows that the learning is a fairly constant process from one individual to the next; it may have differed quantitatively, but the proportions were about the same. In general this correlation would be expected to be fairly high.

5. *Order of Learning.* The order of learning of the individual sections was determined by the same criterion as was employed for the whole maze: the successful negotiation of three consecutive perfect trials in that section. Of course the learning did not stop until all sections had been learned. This division was made after completion of the task from inspection of the records. The average trial on which each section was learned is given in table 3.

TABLE 3

TRIAL ON WHICH EACH SECTION IS LEARNED

	Whole Maze	I	II	III	IV
A	29.0	23.2	24.8	21.0	19.9
B	22.3	20.4	20.3	15.3	17.7

The order of learning in terms of number of subjects who learned each section in each rank is given in Table 4, the figures for position B (where the panels are reversed) being multiplied by two to compare on even basis with position A, where twice as many subjects were used.

TABLE 4

ORDER OF LEARNING; INDIVIDUAL SECTIONS

Section	Position A I	II	III	IV
Learned:				
First	7	9	15	9
Second	8	5	10	16
Third	14	16	7	3
Fourth	20	8	6	6

Section	Position B III	IV	I	II
Learned:				
First	26	14	8	4
Second	8	16	6	10
Third	2	8	14	6
Fourth	2	0	16	20

Weighting and summing these raw scores both as to section and position we come to these conclusions: (1) The sections in order of ease of learning are III, IV, I, and II. (2) The positions, eliminating as far as possible other factors, are in order of merit as regards speed of learning 1, 4, 3, 2.

The question arises as to why there is a difference in difficulty of learning between the various sections. Analysis of the turns within the sections shows that this depends on the number of changes of direction, or proportions of sequences in the section. Thus III and IV have five changes, while I has six, and II has eight. The order of learning is just that, with the exception that III seems to have been learned slightly in advance of IV. Altho each has the same number of changes of direction III has two sequences of three like choices, while IV has only one, which may account for the difference. The position of a section also exerts influence; universally when a section was moved from an inside position to an outside one it was fixed earlier in the whole learning process; when it was moved to the inside, learning was delayed.

An interesting point is a divergence from the general rule on the part of the very fast learners. The four subjects who learned the maze in under ten trials learned in progressive order. The learning was so rapid that differentiation was rather difficult and arbitrary, but where mistakes were made was on sections toward the end. Apparently these subjects devised such good methods or had such good memories that the difficulties inherent in the sections do not present trouble to them in proportion.

Now that we have found the outside section to be learned earliest, let us see if the outside turns of each section are likewise at an advantage. Warden found that they were, all his results being derived from a single section maze, and postulated principles of primacy and recency as factors in maze learning. The trouble with splitting up an individual section is that often a sequence is divided, throwing the many errors made on the last turn into the next division. But we have here four sections, so these discrepancies should balance each other. Each section containing ten turns, the divisions are made as follows: 1 to 3 in the first; 4 to 7 in the second; and 8 to 10 in the last. In the first an average of 981 errors per blind for all four sections is made; in the second there are 1422; and 1233 are made in the third. So we conclude that the first part of each section is favored; then the last; and finally the middle part. This is in exact accordance with the results we have found for the individual sections of the whole maze, and also with Warden's (3) results. This treatment of order of learning thru study of the frequency of errors is not a direct method, but ascertaining the order of elimination is an impossible task in this case, owing to the great individual differences and fluctuations. It is felt to be justified, furthermore, by the high correlation found between errors and trials.

6. *Methods of learning.* Before or during the learning period no suggestion was made to the subjects about using any one method of learning. The nearest approach to a suggestion came when a subject would ask the experimenter how he was to learn; he was merely told to proceed in his own way. After he had finished he was asked how he had learned, being allowed to describe the process in his own words rather than attempting to force the responses into definite predetermined categories. Also he was asked if he had used any further method secondarily, and if he had discarded any method as seeming unsatisfactory for an ultimate solution of the learning. The methods reported by the subjects are classed under general heads as far as possible.

TABLE 5

METHODS OF LEARNING FOUR SECTION MAZE

Method	Number Using	Trials
Verbal	29	19.5
Visual	4	24.3
Combination: Verbal, Visual & Motor	3	27.7
Verbal; Another Discarded	7	30.3
Verbal; Another Secondary	7	31.7
Motor	10	44.5

Average for all methods: 26.8

The terminology for the main heads is borrowed from Warden (4), as his nomenclature seems highly satisfactory. We have added a few headings, however. It will be seen that the verbal and visual methods are the only ones below the average in number of trials to learn. Let us examine briefly each method in order of its efficiency. (1) The verbal method, which takes some form of counting the turns, is by far the most satisfactory manner of solution. In many cases the subject would take one or more very slow trials to count very accurately the turns, making the learning very abrupt in regard to the rate at which the errors decreased. (2) The visual method ranks second, but it is doubtful whether it is as efficient as appears. Many tried it but discarded it in favor of a verbal solution, finding that it would be impossible to form such a large pattern by visual imagery. Only in exceptional cases could a person learn in this manner. (3) A combination of all three methods ranks next. Probably one predominated over the others, but it was inadvisable to force the subject to make a choice. (4) The verbal method with some other method used for a considerable number of trials and then discarded is next. (5) The verbal method with some other employed secondarily is fifth. Usually this took the form of the easier portions being fixed in a motor manner and the rest learned by counting. (6) Motor learning is the least efficient, taking far more trials than any of the others. This is essentially an unforced, unconscious learning process, without particular attention being paid by the subject, so it would be expected to waste many more trials. The saving element on the subject's patience was the fact that there was little strain to concentrate, and also that the individual trials were done very fast. This allowed many more in an hour than is possible where the subject is actively

thinking of the pattern, trying to remember for the succeeding trials. Often where subjects were particularly delayed in learning one portion of the pattern, the parts learned first were relegated to the motor manner of running.

An interesting point is that several subjects of around thirty-five years of age were used and all had considerable difficulty and emotional strain, altho they had good records in learning. All were advanced graduate students. The theory of their difficulty is that by this time in life they are so dependent on visual imagery that deprivation is much more confusing than it was to the younger subjects.

7. *Time* was kept of all trials, but it shows very little, at least along the lines of our treatment. Part of the reason for this was that the subjects were told before starting to ignore this element; to stop and think over the pattern when they wished, during or between trials; that the emphasis was laid on accuracy and fewness of trials. Even without this warning the majority of subjects soon realized that racing thru, which characterized the motor method, was uneconomical. The chief feature which time shows up is where a subject changes from one method to another. This usually consisted in dropping the motor method in favor of counting the turns. After a few fast trials the record shows one or two very slow ones, which indicates close and careful attention is being paid to the pattern. Time on the perfect trials may indicate the confidence the subject has in his ability to get thru the pattern successfully.

Conclusions

1. The outside sections tend to be learned before the inside ones. Furthermore the first section is favored over the last. The reversal of the sections proved that the results were due to the position, and not to the relative ease of learning the sections themselves. This verifies Warden's principles of primacy and recency, perhaps better termed firstness and lastness.

2. The tendency stated in the preceding paragraph is somewhat complicated by the fact that all sections are not of equal difficulty. It was found that the sections vary in ease of learning according to the number of changes of direction; the fewer changes (or the more sequences) the earlier the particular section is learned. The reversal of sections again confirms the conclusion.

3. Four sections are not correspondingly more difficult to learn than one. They require 26.8 trials as contrasted with 16.7 for the single section. So it is concluded that the chief difficulty lies in

devising a proper method of attack, rather than in actual memorization of the pattern.

4. The frequency of individual errors is found to depend on sequences largely. An increasing number of errors is found in each successive blind in a sequence of like choices. The most frequent errors also tend to be most frequent in individual subjects, a good uniformity being shown; and are naturally also the last to be eliminated.

5. Ranked in order of efficiency in learning, the methods of attacking the problem are: verbal or counting, forming a visual pattern, a combination of verbal, visual, and motor, verbal with some other method discarded before completion of learning, verbal with some other method employed secondarily, and motor last.

6. The multiple-T maze with the type of pattern first described by Warden and used here is advocated as it is uniform in design, and presents similar situations to the subject on successive choices. Further, it is capable of a high degree of statistical analysis.

7. The elevated finger maze is decidedly easier to learn than the same pattern in a stylus construction. This is based on comparisons between Warden's results and those obtained here. Warden's subjects required an average of 69.4 trials, while ours took only 16.7 on the same pattern, the only difference being that in this the path was followed with the finger. Both groups of subjects seem comparable in ability, so the discrepancy appears valid. The errors are very closely proportionate in both. The difference is explained as due to the cutaneous experience afforded by this type of pathway, to the fact that the finger covers the whole pathway and no possible entrance can be missed, and to the speed of the individual trials permitting the building up of a more integrated pattern.

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THE ORIGIN OF ERROR

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I. MODERN ATTITUDE TOWARDS THE TOPIC

Few facts, one might have thought, could be more fascinating for the psychologist than the proneness of the mind to fall into error. To the observation of this propensity, more perhaps than to any other single cause, may be traced the very beginning of all profound reflection about mental power. Witness that culminating masterpiece of Plato, his Theaetetus! And the long ages that have since flowed by have continually invested the matter with further interest. For Aquinas, Scotus, and most other Scholastics, the problem as to how belief ever comes to be led astray was cardinal. And with the champions of the Renaissance and Post-Renaissance schools—Descartes, Malebranche, and Spinoza—Locke, Berkeley, and Hume—the explanation of error became one of the main planks in their doctrinal platform. Nor was the topic suffered to fall into neglect even by the chief schools that arose afterwards. To it was accorded a prominent place, not only in the Criticism of Kant, but also in the Associationism of Bain.

And indeed, such distinction accorded to it would seem to have been amply deserved. For the part played in mental life by error is beyond all doubt one of extraordinary moment. Its significance is by no means confined to the deep divings of metaphysics. Nor is its attractiveness limited to such dramatic instances as illusions and delusions, the miracle-working force of suggestion, or the products of sexual fabulation. On the quantity and quality of errors committed depend the most familiar and indispensable methods of everyday experimental work, such as those used in determining sensory thresholds, or in investigating memory, or in measuring 'intelligence'. Even animal psychology is throughout affected by the same influence, as is exemplified by the very name of the 'trial-and-

error' procedure, which has been so largely exploited. As for the most striking of all features in modern psychology, namely, its application to practical purposes, here the importance of error would appear to become, if possible, still more dominant. Take education. Look at any schoolboy's exercise book. Alongside of the black ink which sets forth what he hoped would be correct will probably be found red ink—hardly less abundant—which indicates what actually was found to be incorrect. Or take industry. Is not that man the most successful who makes, or sells, or buys, in the right manner rather than in the wrong?

But although all these facts would seem calculated to bring the topic of error to the very forefront of psychology, strangely enough the contrary has actually occurred. Crammed as psychological writings are, and must needs be, with allusions to errors in an incidental manner, they hardly ever arrive at considering these profoundly, or even systematically. Whoever would seek for any well digested information about them may make the whole round of current psychological textbooks. He may fly off in the tangent of ultra-behaviorism; or grub into the 'twitches' of extreme structurism; or join in the wildest pursuit after 'wholes' and 'unity'. For all this, he will become as regards the present quest—that after the nature and origin of error—but little the wiser for his pains.

Moreover, this omission is far too universal and far too thorough to be explicable by any mere neglect. It stands, rather, upon the ground of deliberate intention. Psychologists positively decline to investigate error. They regard it as not being 'their job'. They leave its treatment to philosophy, or more particularly to epistemology.

And—barring the fact that the job is neglected by philosophy also—this attitude of disavowal might seem to merit only praise. For such scientific status as psychology has ever really achieved has always been at the price of renouncing its claims to deal with those great problems which we call philosophical.

How, then, are these two conflicting mandates going to be reconciled? On the one hand, psychology is faced by the plain duty of making error one of its principal studies. On the other, it is warned that the whole topic lies outside its competence.

The solution of the difficulty seems to be supplied by remembering that here, as everywhere, science and philosophy treat of the very same things, but at different levels. Thus, physics describes the nature and laws of matter upon its own independent authority; not the smallest of its findings is ever upset by philosophical criti-

cism. Nevertheless all of them, great or small, are eventually submitted to philosophy for final valuation. Upon physics alone, for instance, devolves the right to decide whether or not the mass of an electron is zero. But philosophy alone can say whether the whole conception of electrons, ether, etc., is anything more than a pragmatic makeshift, a counter of temporary service in place of genuine knowledge.

To psychology, then, in similar manner, fall the exclusive right and duty of settling what respective mental operations actually occur when any person entertains a belief that is either erroneous or its converse, truthful, using these terms in the most ordinary sense 'without prejudice'. Whereas the philosophers, for their part, enjoy the privilege of reconsidering, re-valuing, and even perhaps revolutionising, the very concepts themselves of 'truth' and 'error'.

Nor need we, it may be added, trouble ourselves over much with the objection sometimes raised, that even the indispensable preliminary to an investigation of truth and error is impossible: that of deciding which beliefs are which. For on our present empirical level, the question is, in the great majority of cases at any rate, quite well settled by the unanimous verdict of all sane arbiters. And as for the small residuum which still remains refractory, we can hopefully enough leave the solution to be achieved in due course later on.

II. Illumination from the Doctrine of Noegenesis

Having in this way staked off the scientific field at issue, let us proceed to examine the facts that lie within its limits. On these the most searching light would appear to be shed by the doctrine of 'noegenesis'. For this—alone among the current expositions of psychology—lays down in a perfectly definite manner the laws and processes whereby belief is derived from intuitive evidence. And in so doing, this doctrine also indicates conversely the laws and processes within the sphere of which alone there is scope for error to arise.

The first law affording intuitive evidence is in action when any person, by virtue of living through any experience, thereby knows the nature of any item in it. The second law having a similar virtue comes into play when a person, by virtue of mentally presenting any two or more items, therefrom educes some relation between their respective natures. And the third law of this kind is active when a person, by virtue of presenting any item and a relation, therefrom educes the nature of the correlative item. By

the exercise of these three laws, and by no others, we attain to what we call "truth". The first law combines an item as presented with an item as lived. The second and third combine items together in their mutual relation.

How, then, do we fall into 'error'? Here also, items are combined, and indeed in forms quite analogous to the three that, as we have seen, lead to truth. The sole difference is that in the case of error the combining of the items is effected by some means that does not—like intuitive evidence—ensure their fitting each other.

Of what kind, we have next to ask, can possibly be this further agency by which misfitting may be originated? Let us look what further laws exist, to which we have any right of appealing. We find that, in addition to the three which have just been considered and which are purely qualitative, there exist five more laws, these for their part being all purely quantitative. They may be summarily characterized as those of 'energy', 'retentivity', 'fatigue', 'conation', and 'primordial potency'.[1]

At first sight, the outlook might still appear to be unpromising. For how can these influences of bare quantity ever account for errors which involve misfits of quality? Granting that they may be able to explain faults of omission, how can they touch those of commission? Admitting that they show ground for right items and relations being absent from the mental presentation, in what manner can they explain the fact of wrong ones being present?

And no doubt this objection goes a long way, at any rate with four out of the five laws. Take, for instance, that of mental energy, according to which the activity of the mind tends to maintain a constant output. If the amount available for any particular cognitive operation is sufficient, then this operation may be duly achieved. If insufficient, it cannot be achieved. So far as this is concerned, nothing like error can creep in. Take next the law of fatigue, according to which the occurrence of any cognitive event produces a tendency opposed to its occurring afterwards. The worst to be expected from such a tendency is again not error, but only impotence. Turn to the law of conative control; by this, the conation governs the intensity with which any item or relation is present in consciousness. No mere excess of this intensity, it is

[1]See "The Nature of Intelligence" by the present author, 1923, ch. IX.

obvious, can possibly transform the item from being right to being wrong. Nor can any lack of such intensity do more, at its worst, than extinguish the item or any cognition based upon it. And nearly as much may be said of the law of primordial potency. For the best established influences that come under this heading are innate ability and state of health. High degree of these can only do good; low degree can only tend to obliterate the cognition altogether. To make this law responsible for positive error, there would have to be invoked some further, not yet discovered, purely physiological influence, and one that somehow tends perversely to lead cognition astray.

But there yet remains the fifth law to be taken into account, that of retentivity, according to which the occurrence of any cognitive event produces a tendency for it to occur afterwards. Here, all that we have been noting about the other laws would seem to become reversed. Against this one, no errors of omission can be charged at all (at least, not directly); for to retain and to omit are opposites. But errors of commission—such as modification, deviation, or bias—have scope to unlimited extent. For any item or relation whatever may fit their original context perfectly well and yet, on being retained when this context changes, may result in presenting any degree of misfit.

To this point, then, we are easily brought by virtue of *a priori* consideration. The sole current doctrine that appears to offer any foothold for explaining the origin of error is what has been called that of noegenesis. And within the scope of this doctrine the sole influence that seems at all adequate for any such explanation is given in the law of retentivity, according to which the occurrence of any cognitive event produces a tendency for it to occur afterwards.

Let us next betake ourselves to the *a posteriori* standpoint. We have to examine whether in actual fact any error can be attributed to the influence of retentivity; whether all error can be so attributed; and inversely, whether retentivity of all kinds is capable of generating error.

III. Error due to Reproduction replacing Eduction

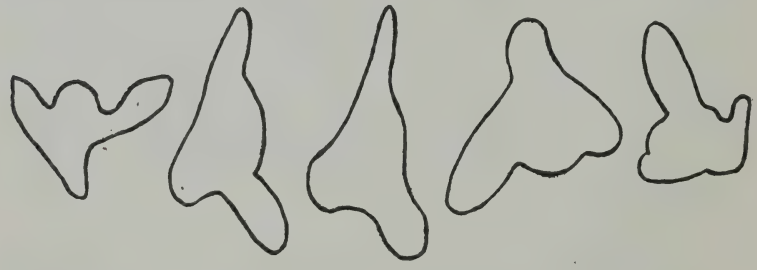
Now, the law of retentivity has been shown to require analysis into two laws; the one is that of 'dispositions'; the other, that of 'inertia', 'lag', or 'persistence'. The former of these two will be

treated by us first. It has been formulated as follows: "Cognitive events by occurring acquire a disposition to recur subsequently".[2]

The effect of such a disposition is obviously reproductive. For in so far as the mental energy follows the lines of the disposition, it must reproduce the same mental content as occurred before. This reproductive tendency has long been one of the main pillars of psychological science. We need not enter into any details about its forms of manifestation, except that—as already remarked by Plato— these forms fall into two distinguishable kinds: reproduction by 'resemblance', and that by 'contiguity'.[3] As to whether these two are eventually reducible to one and the same thing is a difficult problem which need not trouble us here.

The point which does concern us is that liability to error must needs ensue whenever an item arises in any context by virtue of reproduction, but is then taken as being there by virtue of intuitive evidence such as invests the three processes of noegenesis. And opportunity for such displacement of an item is afforded in abundance, since the said three processes—especially the second and third or 'eductive' ones—are intermingled with reproduction in a very intimate manner.

Of the numerous ways in which this intermingling can happen, none perhaps is wider in scope or more straightforward in course than that which was recently found by Stevanovic to constitute the leading characteristic of the operation of 'judging'.[4] This investigator commenced by making his subjects generate concepts by the well known method which we owe to Aveling. He then tested the power of the subjects to pass judgment on the concepts. Suppose, for example, that all the following figures—which are geo-



[2]See *loc. cit.* on page 32.

[3]As to the inward nature of this, see the remarkable work of Aveling on 'Mental Association', *Proc. Aristotelian Soc.*, 1927.

[4]*Brit. J. Psychol.*, Mon. Suppl., 1927.

metrical projections of the same original figure— had acquired the name of "Bogil". Subsequently, the subject would have to supply the missing last word in such sentences as "First Bogil is . ."; "All Bogil are . . . "; "Some Bogil are not . . . ".

Now, it turned out that this task of filling in the last word could be performed in two typically different manners; either eductively, or else reproductively. In the former case the subjects, we are told, "do not employ any verification in satisfying themselves with regard to certainty." But when, on the other hand, the item had been obtained reproductively, the subjects were uncertain of their judgments and "showed a tendency to revert at once to a *verification* of the obtained items." Such a felt need that the reproduction should always be "verified" indicates clearly enough that, if not so verified, it may lead to error.

And that this may actually befall the judgment is illustrated by the following recent experience of my own. An adult person asked by me "How much is one times one?" responded hastily "Two"! The question probably evoked in her mind some very vague idea whose salient points may be indicated by " . . . one . . .one . . . ". Thence, of course, the reproductive transition to "Two" is obvious enough.

More cogent are the examples that may be culled from the experimental investigation just published by Strasheim.[5] In a part of this, the subjects were shown how to behave in a situation of little difficulty. They were then confronted with a series of nine other situations where the appropriate behavior always involved the same essential relation as before, but this had to be applied to more and more unlike fundaments. The aim of the research was to ascertain how much and in what manner the wisdom gained in the earlier situations was utilized in the later ones.

Thus, one series of situations hinged upon the relation of "deciding by chance". It was initiated by a story of two children who were going to pick up sides for a game of football and wanted to settle which of the two should have first choice. They managed this by one of them taking two strips of paper unequal in length, holding these in the hand in such a way that only the tips should be visible, and then getting the other child to 'draw' for the longer piece. The later situations in this series may be exemplified by the following one of medium difficulty.

"Some girls were one day walking along the street when they saw a box full of matches lying on the ground. All rushed to pick it up and two of the girls got it at the same time. So it was decided to toss

[5] A New Method of Mental Testing, 1926, pub. by Warwick & York.

to see which of the two should have it. But no one had any money to toss with and they could find no paper, as the street was quite clean."

Now, the result of this investigation was to show that, instead of educing and subsequently utilizing the needful relation,

"the subjects—especially the dull ones—tended to merely reproduce the procedure of the experimenter."

Thus one subject under five years old picked up the two pieces of paper, but did not make one shorter. On being encouraged to go on, she hesitatingly tore a piece from the top of one bit of paper, but made no attempt to arrange them so as to make them look the same; the shortened piece only just emerged from the hand whilst the other protruded a long way. On some further urging to go on,

"*she herself* chose the longer piece—clearly a bare reproduction of what had happened when the experimenter showed the procedure.

Such reproductive tendency found in all very young children however bright continued with dull children till much later in life. Thus, among the dull of ten years old—

"Not one was able to see that it was necessary to hide the two pieces properly if the experimenter was not to see which was the winning one." One girl "seemed to take great care in arranging the pieces. But the way this was done showed very conclusively that she was merely reproducing in minute detail all that she had seen the experimenter do when he was arranging the paper in showing the procedure. Accordingly . . . she left the broken end of the match next to the intact end, so that the difference between the two was quite easy to see; she had failed to turn the matches round, with the result that the unbroken bottoms projected."

In another part of this investigation, the subjects had to thread a number of mazes drawn on separate pieces of paper, which are described as follows.

"Though differing in details, all were so drawn that there were two parallel lines, about ⅜ inches apart, running horizontally across the middle of the paper, and parallel to its bottom edge. These two lines came to an end at the opposite end of the paper or somewhere upon its surface. They formed a central 'path' from which other 'paths' emerged at right angles and ran in varying directions to the upper and lower edges of the paper, or to an end somewhere upon its surface. No single maze was an exact copy of any other. Indeed, the difference was always considerable so far as external appearances went, since even the sheets of paper on which they were drawn were not all of the same size or shape. See the following diagram."

Diagram I

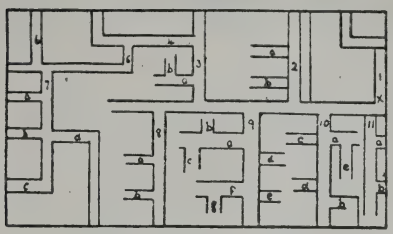
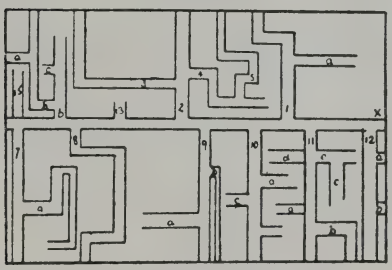


Diagram II



The author proceeds to say that the solution of these 20 mazes really depended upon the eduction of the relations embodied in the following rule:—

"Follow the straight path to the end unless there is an opening to the left. In such a case take the first to the left to its end, unless there is an opening leading forward (or—if one thinks of oneself as walking down the path—to the right). Follow this to the end, unless there is an opening to the left, in which case the first one is to be taken."

The development of this rule broke up into four stages. In the first the subject had only to go straight ahead to the end. In the second stage some turnings to the left were introduced, and the subject had to take the first of these. In the third stage, the said first turning to the left presented later on a further turning forward. In the last stage, this turning forward offered eventually a turning to the left again.

The general result was to prove that—

"The great difference between the 'Dull' and the 'Bright' testee was that the former relied mainly on reproduction, while the latter made use of eduction . . . . Some of the Mazes possessed what we called 'similar paths'. These proved a big stumbling block in the way of rapid learning to the 'Dull'. With few exceptions they are able to memorize the correct paths in the five principal mazes as quickly as the 'Bright'; but in these mazes (those with 'similar paths') they continually come to grief, and

their false move is always due to the influence of reproduction; they go straight for the 'similar path' which they had found correct in the immediately previous maze."

Furthermore, the genuine cases of 'trial-and-error'—

"are such as occur when those operations which have been called 'noegenetic' cease, and are replaced by the anoegenetic ones, and in particular by those belonging to the reproductive class."

Especially interesting is the author's following conclusion—

"Reproduction is the earlier and more primitive process. This seems to be borne out by our results which show that it is the older 'Dull' testees and the younger 'Bright' ones who give the most frequent examples of its occurrence. And when we add the further fact that the higher and developmentally later process (eduction) is naturally the more difficult and therefore the more unpleasant of the two, we can easily understand the aversion for such processes."

Let us now turn to the work of another investigator of 'situations', Laycock. All different kinds of these were classified into three types, 'psychological', 'spatial', and 'mixed' respectively.

As representing the first type Laycock devised the 'Burglar test', where the situation was as follows.—

"Mr. Taylor had a lot of work do to, so he stayed at his office late one evening and, on raising his eyes from his work, he found a burglar standing in front of him with a loaded revolver pointed at his head. He was ordered to put up his hands and was forced to do so. There was a large amount of money in the cash box and, if the burglar got away with it, Taylor's business would be ruined. What was he to do?"

To typify the second or 'spatial' kind of situation, Laycock arranged his 'Models test'.

"It consisted of rectangular pieces of board, eight by ten inches, and a front view showed various buttons, slots, wheels, openings, etc. These different parts were so related that the movement of one of them affected the others in various ways. The general method was to show the pupil the front of the model and, after various movable parts had been manipulated, he was asked to draw what *the back* looked like. In other words, he was asked to educe the correlate to what was seen in front."

The third or 'mixed' kind of situation was represented by the 'Cyrus test' constituted as follows.—

"In the year 559 B. C. Cyrus, King of Persia, set out to capture Babylon. Babylon was a great and mighty city situated on the great river Euphrates which flowed under the walls through the city. The walls were very thick and were 300 feet in height—that is, they were as high as the clock tower of Big Ben at the houses of Parliament. When Cyrus arrived, he found the gates shut, and the guards on the walls looked down and laughed at the Persian army while the rest of the city paid no attention to his coming and gave themselves over to merriment. What was Cyrus to do?"

On trying out these tests, it was found that the subjects—with extremely few exceptions—failed to solve any of these situations in a satisfactory manner when first confronted with them. They were then divided up into five groups, each of which received a training by means of a series of situations which became increasingly similar to the original one. Finally, they were once more confronted with the original situation itself; the effects of the various training procedures were ascertained and compared.

The most general result of the research was to show that erroneous behavior derived mainly from employing reproduction in place of eduction.

Prominent in the responses of the subjects, the author says, "was the reproduction of methods of past sieges without any attempt to determine their suitability in the Cyrus situation. Here the pupils reacted to the situation as a whole, namely to the idea of siege. There was no attempt to analyse the situation into its sub-problems. By associative reproduction the idea of 'siege' called up a past situation where a relation and correlate had been educed for the pupil by a teacher or book or picture at the Cinema". "The reliance upon associative reproduction seemed to be the great cause of the errors made, and that process, together with the tendency to substitute it for the process of eduction, may be given as the prime source of failure in the Cyrus test."

Similarly, as regards the 'Burglar test'.

"Some pupils seemed to rely almost wholly on associative reproduction. They reacted to the situation 'man confronted by a burglar' as a whole, and to this idea they responded with a method linked up with it by previous experience. No attempt was made to analyse the Burglar story into its constituent problems, and answers were given which were expressly ruled out in the test itself."

So too, once more, with the 'Models test'.

"A considerable number failed in the test because of reproduction of methods from their past experience in the form of toys, cameras, magic lanterns, machines, shutters, etc."

In his general conclusions, the author reiterates the same thing.

"Over and over again we have found that the origin of all error lies in replacing the belief derived from the qualitative noegenetic principles by that which comes from the anoegenetic processes, particularly in the form of reproduction. Those children who were considered 'dull' by their teachers and who did poorly on the intelligence test tended greatly to rely on reproduction. The tendency, however, was also found among bright pupils, especially where the association had been strong and the bias towards using the reproduction very great.

Finally, however, the author warns us against the exaggeration

that the reproductive processes always do harm and never any good.—

"Reproduction is a two-edged sword. Not only was it found to be responsible for all error, but it was also found to be very valuable in making past experience and past relations available at all. There remains one other valuable use for reproduction and that lies in the reproduction from past experience of general methods of attacking problems. While a pupil's eductive power seems to be an innate capacity, he can none the less be trained in methods of attacking new situations—methods of analyzing out a problem, of testing items derived from reproduction by eductive processes."

IV. Error due to Reproduction displacing Remembrance

We will proceed to another great class of errors, those which occur in the course of trying to 'remember' anything. About the essential nature of remembrance there has been not a little confusion and controversy. Here we will lay down that the word is properly applied to any system of presentations which seems to the rememberer to constitute an immediate knowing of his own past; either his previous personal experience as such; or else his previously accumulated store of information.[6]

False remembrance, we here maintain, arrives when any item in any part of this system becomes displaced by any item reproduced from elsewhere.

For example, a schoolboy has been known to state that—

"an appendix is a portion of a book which nobody has yet discovered to be of any use."

Obviously, the anatomical system of ideas that should have been remembered in connection with the word appendix has been partly ousted by other ideas which the same word has simultaneously recalled from his bibliographic system.

In the foregoing example, the displacement was from one store of information to another. Quite as easily it may be from one period of personal experience to another. An instance may be supplied by the following report of Plüschke.[7] One day this school-teacher missed a trinket from the chain of his watch. A pupil remarked to him, "Sir, you had it on you still when you took off your overcoat after the interval." Plüschke asked if any other children in the class had seen the trinket on him that morning. Out of the 34 children present (12 to 14 years of age) 5 answered positively in the affirmative. Since he was on that day teaching several other classes, he put the same question to each of them; and each

[6] "The Nature of Intelligence" ch. XIX.
[7] Beilage zur Preuss. Lehrerztg, 17th year, No. 3, 1902.

time he received similar responses. On the whole, out of 243 children 88, or 36.6%, stated quite definitely that they had seen him wearing the trinket that very morning. All declared that the trinket was rectangular and golden, both of which statements were correct. Several gave further details—all verified as having actually happened—about the particular occasion when the trinket had come to their notice. Thus, one boy remarked that he had lost some money himself during the interval and had come up to the teacher to report this fact; in so doing, he said, he had noticed the trinket to be still in place. Eventually, however, the real fact turned out to be that Plüschke had left the trinket in a restaurant on the previous day! Plüschke explains the case as follows.

"Obviously, it was habit that had led all these pupils to make the false statements. At the moment that I asked them about the matter, a presentation arose in their minds of what they had seen previously. This presentation was so vivid, owing to the long habit of perceiving the trinket, that it caused the limits of time to be overlooked and thrust the earlier perception into the immediate past as if it had only just happened."

As another typical instance of the class which we are here considering may be mentioned the results of the experiments of Müller and Pilzecker on the memorizing of meaningless syllables. By these investigators the errors committed were chiefly ascribed to what they called 'active substitution' and 'passive substitution'. In the former of these a given item *a* reproduces another item *b* although it had not really accompanied this; the reason being that some other idea A similar to *a* had done so; in other words, the influence of *a* is displaced by that of A. For example, the sensation of violet can easily in the mind of a child reproduce the word 'blue', the sequence of events being as follows: first, of course, the sensation of violet; then, by virtue of similarity, the re-activating of an old disposition to present blue; and finally, the association evocation of the word blue.

The other or 'passive substitution' occurs when the idea *a* has been associated with the idea *b*, and nevertheless actually reproduces some other idea B which is only similar to *b*. For example, the correct responses "dar", "köm", "fat", "sur" and "süp" have been displaced by the incorrect but more familiar syllables "das", "kom", "fet", "zur" and "sup".

Turning to the case of remembering meaningful material, this has been the topic of an elaborate but still unfinished investigation in our laboratory by Bradley. Variously constructed passages of prose were read out to the subjects, and subsequently these had the task of recalling what they had heard. The ensuing errors, as

might be expected, were very numerous and interesting. But the detailed account and the scientific evaluation of them will be reserved for the pen of Bradley himself.

Before quitting the whole region of erroneous remembrance, it should be noticed that here not only the invading system, but even the system invaded by it, derives essentially from a reproductive operation. For evidently, to remember anything involves somehow the reproducing of it. But still there is a great difference between the two systems. For the invaded one claims belief in its own right, on grounds common to all correctly executed remembering, such grounds being ultimately justifiable even noegenetically. Whereas the invading system only acquires belief by dint of seeming to belong to the one invaded; its plumes are but borrowed property.

V. Error due to Mental Persistence

So far, we have been considering only reproductions; and these derive from the mental tendency to establish dispositions. But this tendency, as already noted, is only one out of two ultimate laws of mental retention. The other law has been called that of 'lag', or 'inertia', or 'persistence'. It has been formulated as follows. "Cognitive processes always both begin and cease more gradually than their (apparent) causes."[8]

To take a familiar example, if a torch is rapidly swung around in a vertical plane, the lighted end will not appear to any spectator as a single star, but rather as a comet followed by a tail. When the revolution is made faster, the tail lengthens; until eventually it extends back to the lighted end again, thus constituting a complete luminous circle. The well known explanation of this phenomenon is that the ray from the lighted end of the torch to any point in the retina produces an excitation there which continues for some time after the ray itself has passed further along the retina. Turning from this example of sensation to one of thought, everybody knows how a piece of news communicated, or an order given, or a problem studied, is apt to remain for some time afterwards still vividly persistent in consciousness. So much so, that there may upon occasion be much difficulty in thinking about any other matter.

To distinguish such instances of persistence from the previously examined case of dispositions may often be difficult in actual practice; there may be much doubt whether any particular item in the mental field has been continuously there, or had disappeared and

[8]See *loc. cit.* on p. 32.

then re-appeared. But there is much evidence to indicate that the two cases are at bottom fundamentally different. Foremost here has been the comparison of mental abilities of different individuals. For this has shown that the tendency to strong inertia is a general characteristic of an individual in all kinds of performance; whereas the tendency to form strong dispositions is *not* a general characteristic.[9] The distinction between the two cases is further stressed by theoretical and physiological considerations. For the establishment of disposition may be conceived as some tunneling out of channels in brain for the passage of energy; whereas the persistence may be taken to derive from the inertia of this energy itself on being diverted from one set of channels to another.

Despite all such unlikenesses in other respects, however, the case of persistence and that of dispositions appear to be quite alike in respect of producing the phenomenon now concerning us, namely, error. In order that an item should stray into a wrong place, it seems to make no difference at all whether this item had been lingering in consciousness by virtue of the lag of the mental energy, or had arisen anew into consciousness by reason of the mental energy being attracted into preformed channels.

An instance of the lag may be taken from an experience of my own which happened just before writing these lines. Whilst lecturing on some recent experimental investigations, I mentioned a work of Kingsley, writing his name as usual on the blackboard. Next, I said that I would turn to a work of Dickinson, and I proceeded—as I believed—to write this latter name under the other one. To my own surprise, I found that I had commenced the Dickinson with a K.

As another instance, this time resting upon the more solid basis of experiment, we may take the research executed by Buchholz under the direction of Wirth.[10] A dot was exhibited for a fraction of a second; then, after two seconds, another dot was shown. The subjects had to judge whether the first dot had lain more to the right or to the left, or higher, or lower, than that seen subsequently. During part of these experiments, the said two dots compared with one another were preceded by yet another dot, with which, however, the subjects were told not to concern themselves. Despite this instruction, such a previously displayed dot did influence the judgments; if it had lain a little to one side of the dot given next afterwards, it caused the apparent position of this latter to be diverted a little in the same direction.

[9]The Abilities of Man, by present author, 1927.
[10]*Psych. Stud.*, IX, 1914.

Of similar origin appears to be the fact discovered by Mach, that two time-intervals which are distinctly different on first comparison will nevertheless on being repeated become gradually more alike and finally seem quite equal.[11] This observation was subsequently corroborated by Schumann.[12] Such a tendency towards becoming alike was still more clearly manifested in the experiments of Nichols. His subjects had at first to mark time to six strokes of a time-apparatus; then, with the aid of this apparatus, they had to go on reproducing what seemed to be the same intervals of time as before. It turned out that this reproduction of the intervals was markedly influenced when the subjects had heard some strokes of the time-apparatus at a different interval *before* the whole experiment just described. According as these earlier intervals had been longer or shorter than the subsequent six, so too the final reproductions tended to become larger or shorter than they.[13]

Analogous, further, was my own experimental result, that the apparent position of any tactile stimulation of an unseen limb always tends to be biassed in the direction of any preceding stimulation of that limb.[14] Much earlier still was the experiment of Wundt, in which he touched his subjects with the two points of a compass simultaneously. When he did this twice in succession, letting the points be somewhat more distant from each other the first time than the second, the distance the second time was judged to be greater than it was usually judged. Conversely, when the points were somewhat nearer together the first time than the second, then the distance the second time was judged to be smaller than usually judged. The whole phenomenon is summed up by Wundt in the following law: "Every preceding idea (Vorstellung) tends to alter a following one in like direction."

VI. Anticipatory and Simultaneous Influences

Of just the same nature is really the case where, instead of an earlier item influencing a later one, the reverse seems to occur. For example, when intending to typewrite 'latterly' and believing that one does so, one may in place of this write 'tallerly'; that is to say, the 't', which ought to come later in the word, displaces the 'l', which ought to come earlier. Such an inversion is only possible because the whole word, including both the 't' and the 'l', is anticipated mentally before writing any of it; hereby, either

[11] *Ber. d. Kais. Akad. zu Wien,* M-N Klasse, Ab. 2, 1865.
[12] *Zeit. f. Psychol.,* IV, 1893, P. 4.
[13] *Amer. J. Psychol.,* VI, 1894.
[14] *Psychol. Studien,* I, 1906.

of these letters becomes able to invade any part of the writing. Such a shift of place has long been recognized as cardinal for the history of languages.

Closely akin, too, is the case well known to psychologists in all ages and described with especial vividness by James under the name of 'pre-perception' (adopted from Lewes).

"When watching for the distant clock to strike, our mind is so filled with its image that at every moment we think we hear the longed-for or dreaded sound. So of an awaited footstep. Every stir in the wood is for the hunter his game; for the fugitive his pursuers. Every bonnet in the street is momentarily taken by the lover to enshroud the head of his idol."[15]

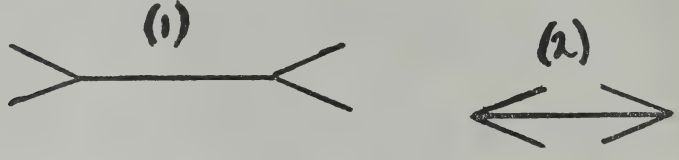
There remains a case intermediate between the two last considered, namely, where the influencing item seems to occur neither before nor after the item influenced, but *simultaneously* with this. An example was afforded by the experiments of Pearce, where the subject was required to locate a point pressed (unseen) against the skin, whilst at the same time a second point was similarly applied eight or ten cm. away. This other point was found to exercise an attracting influence upon the apparent site of the one to be located. Quite analogous results were obtained about auditory localisation. When the subject had to point in the direction of a sound (of unseen origin) whilst at the same time another and easily distinguishable sound was produced a little to one side, the subject's pointing tended to be biassed towards the other and subsidiary sound.[16]

Now, although in such cases the influencing item seems neither to precede nor to follow but to accompany the item influenced, it does really once again precede. For cognitive items are always and necessarily manifested in cognitive processes, and these latter must have more or less duration. Hence even the strictest simultaneousness between these processes must needs involve succession between them also; for the earlier portion of either process must be succeeded by the later portion of the other. Once more, then, the influence can be explained by the principle of retentivity.

Having thus established in general that mental retentivity is capable of explaining how items of cognition can impart something of their own character to other items that are seemingly quite simultaneous, we thereby obtain a master-key to the understanding of error in innumerable situations. For instance, there is the much discussed and contested illusion of Müller-Lyer. This can now be easily explained by supposing that the apparent greater length of

[15]Principles of Psychology, II, p. 442.
[16]*Archiv f. d. G. Psychol.*, I, 1903.

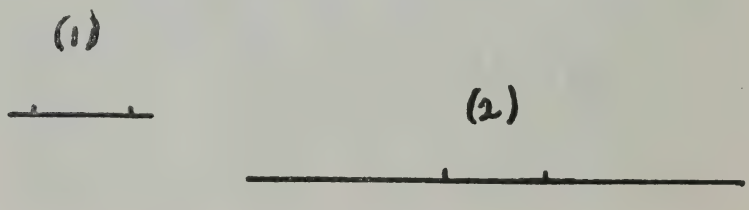
the stem in (1) than of that in (2) has its origin in the greater lateral extension of the whole figure in (1) than of that in (2).



Nor is this explanation weakened by the fact that the illusion occurs even when the figures are exposed together for an extremely brief period, say 1/20 of a second. For then introspection shows that, despite the briefness of the physical exposition, the perceptual operation itself is by no means brief. On the contrary, at least two stages in it can be clearly distinguished. First, the entire figures are seen with diffused mental energy ('attention'); and then one or both of the stems seen with concentrated energy; and only in this second period is it that the one stem seems to be longer than the other.

Herewith we may claim to have at last elucidated a class of phenomena which plays a conspicuous part in the doctrine of 'Gestalt' or 'configuration', especially in the 'unitarian' version of it as developed by the Berlin school. The fact that a part of a figure seems longer when the figure as a whole is so has been repeatedly brought forward in favor of the thesis that 'the whole is prior to the parts'. But—so far as I know—no advocate of this thesis had previously explained the chief point really at issue, namely, *why* the whole should influence the part in this particular manner.

Let us now go on to those cases which are commonly regarded as just the opposite to all that we have been considering hitherto; that is to say, the items occurring simultaneously are influenced in the direction of appearing, not more like, but more unlike their companions. The phenomenon is called that of 'contrast' (not the physiological but the psychological kind). Take, for example, the following two figures.



Most observers report that the line between the vertical strokes in (1) looks distinctly longer than that in (2); and this fact is attributed to the former line appearing long by 'contrast' with the very short lines outside the vertical strokes.

But the fact may be considered in a less superficial manner with aid of the same principles that have been helping us throughout. For the second of these principles shows that one and the same cognitive process includes knowledge both of the items related and also of their inter-relation. In other words, the items are known both absolutely and also relatively. The illusion of contrast can, then, be readily explained by supposing that the relative cognition becomes in some degree displaced over to the absolute one; this tendency may be taken to be strong in proportion as the relative cognition is a striking and impressive one. Thus, in our present instance, a line perceived to be very much longer than another one thereby tends to seem somewhat longer even in itself than would otherwise have happened; the character of 'length' is so strong in the relative cognition that some degree of it overflows into the absolute cognition.

Turning from the sphere of perception to that of thought, here also we find that errors derive in great abundance from the fact that cognitions tend to impart something of their own nature to other cognitions that are effected about the same time. This fact lies at the root of, and may be illustrated from, the different classes of logical fallacy.

Thus, one such class is derived from 'ambiguity of terms'. An example is the use often made of the saying that 'an ounce of fact is worth a pound of theory'. This saying is true enough when the word theory is taken to mean any elaborate speculation resting on few and hasty observations. But then the meaning of this word is shifted over to any elaborate deduction however numerous and careful may have been the observations upon which it rests; the genuine enough likeness in respect of both cases involving elaborate thought is allowed to irradiate so as to make the two seem alike in all respects whatsoever. Such a fallacy, it may be noted, is just the converse to that which we have seen to occur in the Müller-Lyer illusion. For whereas the latter is due to a character (length) being displaced from the whole to a part, the former is due to a character (likeness) being displaced from a part to the whole.

Another class of fallacies that may be traced to a similar source is that of 'false analogy'. Thus, despotic government has been declared to be beneficial on the ground that parental authority is so. Here again the likeness in one respect—this time, that of irrespon-

sible control—is extended to likeness in all respects. And under the same rubric may be brought much of what is perhaps the most harmful of all fallacies, namely, undue generalisation. For instance, the social habits correctly enough observed to prevail in certain spheres of life in Paris may thereupon be taken as habitual all over France. In this manner, the genuine likeness cognised between the provincials and the Parisians as regards nationality irradiates so as to produce an apparent likeness as regards social habits.

But if such undue extension may happen to the cognition of likeness, why not—it may be asked—to that of difference also? And this also, indeed, does really happen. An instance is only too familiar in psychological writings, where a pseudo-originality has often been obtained by merely dressing up old views in new terminology. The genuine difference of wording is displaced so as to become an apparent difference of meaning.

Another character which has been allowed to irradiate with much mischievous effect is that of 'unity'. Thus, a stumbling block for all psychology—and especially at the present day—has been the belief in unitary 'faculties'. Because a class of abilities or tendencies such as 'intelligence' or 'power of concentration' are gathered together under a single name, they are taken to behave as a single function.

Yet another character that has been displaced with unhappy results is that of 'impossibility'. A notorious example is the assumption that whatever cannot be conceived cannot occur. Thus, many a writer used to maintain that Antipodes were impossible, merely because he himself could not picture persons standing up and yet having their heads in the same direction as his feet.

VII. Error ascribed to 'Creative Imagination'

At this point the time has come to face a formidable objection by which our attempt to explain all error might seem to be at once wrecked. For whereas we have been tracing its origin to nothing more than a retaining of something old, both philosophers and common sense have hitherto almost unanimously ascribed it—at any rate in large part—to the imagination of something new.

And certainly this doctrine can bring forward a great volume of supporting evidence, coming especially from the sphere of sensory perception. When a man sees—sometimes even with hallucinatory vividness—a lion's head looking in at the window, or girls' clothes flying off them whilst at their washing tubs, or hears voices up the chimney that tell him to throw himself out of the window,

or feels his length in bed increase to 7 feet and then become normal again, all this might seem to be very much more than any mere reproducing or persisting of what had previously been actually experienced.

Further, all such unlikeness to mere retention is apparently only increased by examining how an act of creative imagination is fundamentally constituted according to the very principles that have been guiding us. For according to these principles all such creativeness consists at bottom in the eduction of a correlate, which process is (or at any rate can be) accompanied by self-evident truthfulness.

The difficulty vanishes, however, on considering that the said eduction only goes so far as to evoke a *mental presentation* of the correlative object. And up to this phase, no error at all enters upon the scene. The man cited makes no mistake when he only believes that he has a visual presentation of the lion's head. Where he goes wrong is in taking the presented head itself to have a real existence transcending his presentation.

Whence, then, does the supervening belief in such real existence derive its origin? To answer this question, we must recall that every sensory presentation of an external object is generated (according to the same principles as all other cognition) out of some primarily experienced sentience.[17] Such sentience itself does possess the character of real existence. But forthwith this character irradiates over to the external object and thus produces the illusion.

VIII. Part played by Conation and Affection

Another great rival candidate to explain the genesis of error—and therefore an opponent of our claim to explain it by retentivity—consists in conation and affection.

And indeed, unquestionably, a very large share of the mistakes that a person makes in life—especially the more pernicious of them—are due to his allowing his beliefs to be swayed by his hopes and fears.

Nevertheless, we must here side with the view that the influence of conation and affection, however potent it may be in degree, is never really the proximate cause of error, but only a remote cause or a favoring condition. No one has expressed this view more cogently than J. S. Mill.—

[17]This fact, indicated in the work cited on p. 32 (ch. XIV), has since been fully corroborated in an experimental investigation of Cattell (not yet published).

"We cannot believe a proposition only by wishing, or only by dreading, to believe it. The most violent inclinations to find a set of propositions true will not enable the weakest of mankind to believe them without a vestige of intellectual grounds—without any, even apparent, evidence. It acts indirectly, by placing the intellectual grounds of belief in an incomplete or distorted shape before his eyes. It makes him shrink from the irksome labour of a rigorous induction, when he has a misgiving that its results may be disagreeable; and in such examination as he does institute, it makes him exert that which *is* in a certain measure voluntary, his attention, unfairly, giving a larger share of it to the evidence which seems favourable to the desired conclusion, a smaller to that which seems unfavourable . . . Though the opinions of the generality of mankind, when not dependent on mere habit and inculcation, have their root much more in the inclinations than in the intellect, it is a necessary condition to the triumph of the moral bias that it should first pervert the understanding."[18]

IX. Error due to Omission

The preceding consideration leads on to a problem of very ancient date and high epistemological importance, though perhaps less vital from the standpoint of psychology. We have throughout been treating error as essentially something positive, something committed. But the question may be raised whether it cannot—or even whether it does not always—consist in some mere omission.

With the last mentioned and extreme negative view of error—ascribed particularly to Spinoza and in some measure to Descartes also—we need not here concern ourselves. It arises only on the higher or philosophical plane. At our present lower level of empirical science at any rate, there can be no reasonable doubt but that cognition may suffer from faults of either kind, commission as well as omission. Every schoolboy realises the difference between answering a question wrongly and being unable to answer it at all.

The sole problem for us, then, is as to how the two kinds of faultiness are causally inter-related. Now, with all three of our principles of noegenesis, faults of omission are powerless to produce any errors of commission; any beliefs attained by virtue of these principles must necessarily hold good, whatever else may or may not be known or true.

But this fact, although agreeing obviously enough with the case of 'deductive' reasoning, is not so easy to harmonise with reasoning of the 'inductive' kind. For merely by omitting all unfavorable instances, one can arrive at any induction however absurd. The planets can be demonstrated to regulate human destinies, every quack medicine can be proved to cure every disease,

[18]System of Logic, 8th ed. 1884.

and any person can equally well be shown to be wise or foolish. As much may be said about all other evidence of the cumulative sort. By merely selecting from the evidence about a crime, the lawyer can argue that the accused man is guilty or that he is innocent. By force of ignoring whatever does not suit his purpose, the most unsound controversialist can at least put up a plausible case.

Granting all this, however, the omission can never properly be said to constitute the error, or even to be its proximate cause. No mistake at all is generated solely by selecting evidence in a partial manner. It does not arise until the indications derived from this part of the evidence are taken to be adequately representative of the whole of it. Omission, then, like hope and fear, has no closer connection with error than that of being a remote cause or a favoring condition.

X. Relation between Displacer and Displacee

There is yet another condition to be taken into account. Up to the present point, we have been content to show that error always derives from the character of some item of cognition being transferred over to some other item. But such transference must evidently depend in large measure from some relation or relations which the two items bear to one another. What, then, are these relations?

In general, the relation most favorable for the transference is that of resemblance. When anyone trying to repeat a passage of prose brings in something from another passage, the influencing and the influenced ideas are much more likely to be akin than quite foreign to each other. Again, the wings in the Müller-Lyer figure exert most influence upon the appearance of the stem when both are constructed of similar lines. Diminish the similarity—as by making the lines of the wings differ from that of the stem in thickness or in color—and proportionately the illusion will diminish.

Still, not all cases seem to be reducible to this type. Take, for example, the grave fallacy where diverse things mustered under the same name are thereupon taken to have the same nature. In this case, the character of sameness is displaced from the name over to the thing named; and yet name and thing have usually no resemblance whatever. The two do, however, stand in the relation of 'contiguity'. Here, then, is a second kind of relation by which the displacement may be effected.

But these two conditions of displacement—resemblance and contiguity—are just the two conditions of reproduction itself.

And when we reflect that every displacement of a character does in fact constitute a reproduction of it, we attain to the result that the relations between displacer and displacee do not after all cumber psychology with anything additional to the laws that have explained to us everything else.

XI. Transfer of Belief

Nevertheless, our explanation of error is still incomplete, and upon a most fundamental point. We have seen clearly enough how any character may become displaced from one item to another; but this fact by itself is quite inadequate to account for the generation of anything really erroneous. For the proposition engendered by the displacement is, by assumption, false. It therefore cannot possibly be believed on its own intuitive evidence. How, then, does it ever come to be believed at all?

Our answer must be that, together with the displacement of the character, there also occurs some displacement of belief itself. By this word is here not meant any entire believed proposition, but rather that which constitutes the difference between a believed proposition (true or false) and one which is not believed (or disbelieved). Otherwise expressed, it is the mental acceptance as distinguished from that which is accepted. Take the case of the fallacy of an ambiguous term. Here, the acceptance originally gained for the likeness that does exist in some respect between the two meanings of the term is transferred over to a presentation of likeness in general, which includes other respects wherein the two meanings really are unlike.

As for the current doctrine that belief can—by means of some 'primitive credulity'—be generated spontaneously out of mere presentative consciousness, this may be dismissed as on a par with the old physiological doctrine that living matter can arise spontaneously out of dead matter.

XII. Conclusion

Summarizing this whole paper, we may conclude that in all cases the primary source of error lies in two special manifestations of the principle of retentivity. The first is that some character is retained over from one item of cognition to another. The second is that there also occurs an analogous displacement of belief.

For want of space, we must refrain from here considering the consequences of this conclusion for psychology both theoretical and

practical, as also for philosophy. It must suffice to say that these consequences appear to the present writer at any rate to be very momentous.

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AN EXPERIMENT UPON REPEATED REPRODUCTION

From the Psychological Laboratory, University of Cambridge, England.

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1. The Experiments

Everybody knows that material which has been learned suffers much change with repeated reproduction during the passage of time. Yet the precise nature and determination of these changes have been comparatively little investigated except in the case of nonsense syllables; and an air of artificiality hangs over most laboratory experiments with nonsense syllables. It is a thoroughly unfounded assumption that the simplification of material must mean a corresponding simplification of reaction. Usually all that it means is that attitudes and reactions are forced upon an observer which are far removed from anything that he does in daily life.

The experiments which I shall discuss briefly in this paper are a part of an attempt to investigate how and why material which has been learned changes with lapse of time, the whole of the investigation being kept as near as possible to the types of situation and reaction which occur frequently outside of the laboratory. Many methods have been used, but at present I shall discuss one of these only. In itself this method has no claim to originality. It has been used repeatedly by various investigators for different purposes, and may be described simply. Graphic, verbal, or other material is introduced to an observer's notice under prescribed conditions. He then describes this material when it has been withdrawn and thereafter, at intervals, attempts repeated reproduction. There is no hard and fast memorisation, for this is rare and in general useless in real life. And no particular stress is laid upon literal accuracy of reproduction, for this also is as a rule of infrequent occurrence and of little value under ordinary daily conditions.

There are obvious difficulties attending the use of this method which would no doubt be a feeble instrument of research were it employed merely by itself. The general plan of the experiments

was to continue to ask for reproductions until the material had, for a given observer, assumed a relatively fixed and final form. If reproductions are effected frequently, however, and at small intervals, the form may become fixed extremely rapidly; while if long intervals are allowed, the process of gradual transformation may continue for very considerable periods. The experiments, as I shall report their results here, give, therefore, only a small section of a lengthy process of continuous change.

Further it is perfectly clear that such changes as occurred were not at all strictly under control. The nature of the changes could be noticed easily, but their conditions were far less open to inspection. For any detailed study of the conditions of the changes illustrated recourse must be had to other methods.

In this paper I propose to restrict myself to a discussion of a few of the general principles which the experiments bring out, and to give only such illustrations as are necessary for the purposes of the immediate argument. I shall not deal with graphically presented material, but with verbal formations alone. All of the latter were given in the form of continuous passages of prose or of verse, some argumentative and general, and some specific and expressed in realistic story form. Here I shall deal almost solely with the reproductions of stories.

The general problem of the experiments can be stated in the following terms:—Given that certain material of a complex nature is introduced to an observer's notice, what determines his reception of such material, and what characteristics mark his several attempts to repeat or reproduce such material?

The stories, argumentative passages, or other material used in the experiment were read by the observers, or related to them. After an interval of fifteen minutes a first attempt was made at reproduction and thereafter further versions were obtained at intervals of increasing length.

2. Persistence of Form in Reproduction

Perhaps the most general characteristic of the series of reproductions obtained was the persistence, for any given subject, of the general form of his first reproduction. With the single, but probably significant exception of a few subjects of a definitely visualising type, by far the greatest amount of change was effected in the early stages of the experiment. It was as if response to the general scheme, form, order, and arrangement of such material is dominant both in initial reception and in subsequent reproduction. Whenever we have to deal with novel material, and are

not concentrating our attention with a view to strict memory work, the most immediately important factor, alike in acceptance and in reproduction, is that of the general impression. A story is, by most subjects, at once labelled as being of this or that type, and in addition as possessing a few outstanding details. The type gives the form, and the striking detail may persist with as little change, for long periods, as the form itself. Other details are altered or omitted. Omissions continue to occur for a considerable time, but other changes are, as a rule, effected extremely rapidly. The form, plan, type, or scheme of a story is, for the ordinary educated adult, the most persistent factor of all. It ought to be possible experimentally to follow the development of this sort of response, and to determine its relative importance in individuals of different age and different intellectual status. It may be that, once this response to the form factor is established, its stability and effectiveness are due to its marked affective character. Both this point and the study of the mechanisms by which transformation of detail are produced are, however, best pursued by a consideration of the parts played by the process of rationalisation in the course of repeated reproduction.

3. Processes of Rationalisation

There is a marked and well-known distinction, both in perceptual and in memory processes, between direct reaction to what is literally present and reaction under the guidance of some tendency which gives what is presented a setting and an explanation. The latter tendency is present to some extent in all perceptual and memory processes, but may vary greatly in importance and prominence from case to case. Sometimes, in these experiments, reasons were definitely and explicitly formulated and introduced into reproductions to account for material which had been presented but not explained. Sometimes, without any definite formulation of reasons, the material was so changed that it could be accepted by the observer without question and with satisfaction. The first process appears to be a special instance of the second. Both have the same function in mental life, and I shall discuss both under the head of rationalisation.

In these experiments rationalisation was applied sometimes to the stories as a whole and sometimes to particular details. In the first case the process expressed the need felt by practically every educated observer that a story should have a general setting. Hardly ever, at the outset, was there an attitude of simple acceptance. Every story presented had to be connected, certainly as a

whole, and, if possible, as regards its details also, had to be connected with something else. This, in a very wide sense, is simply the process of "giving meaning" to what is attended to. But the "meaning" may be extremely indeterminate, and even mainly negative. For example, a number of the tales used were folk stories developed in a community very different from that of my observers. The commonest remark about these stories was, "That's not an English tale". Sometimes the narrative was rendered satisfactory by being called a "dream". "This," said one observer, "is very clearly a murder concealment dream." She proceeded to an interpretation according to approved modern symbolism and the story was, with no further trouble, comfortably accepted.

In fact all incoming material, if it is to be dealt with in any manner must be somehow labelled. A negative label is often enough. When an Englishman calls a tale "not English" he can at once proceed to accept comfortably odd, out of the way, and perhaps inconsistent material with very little resistence. How these labels may be developed and how they can be taken over ready-made from society are matters of some interest, not out of the reach of experimental study; but I must not now stop to discuss these questions.

There are two points to be made about that rationalisation of a story as a whole which stops short at finding a label. The first is that the process is emphatically not merely a question of relating the newly presented material to old acquirements of *knowledge*. Primarily it depends upon the active bias or special reaction tendencies that are awakened in the observer by the new material, and these then set the new into relation with the old. To speak as if what is accepted and given a place in mental life is simply a question of what fits into already formed apperception systems is to miss the obvious point that the process of fitting is an active process depending directly upon the performed tendencies and bias which a person brings to his task. The second point is that this process of rationalisation is only partially—it might be said only lazily—an intellectual process. No doubt the attempt, however vague, to seek out the connexions of things is always to some degree intellectual. But here the attempt stops when it produces an attitude best described as "the attitude in which no further questions are asked". The end state is primarily affective. Once reached, and it is generally reached very quickly, it recurs very readily in reference to the particular material concerned, and it is this, more

than anything else, which seems to account for the persistent sameness of repeated reproduction.

The rationalisation which gives material as a whole its general setting is only a part of what usually occurs. Details also must be dealt with and every chain of reproductions illustrates the rationalising process applied to particular items.

The most direct method is that which provides definite links of connexion between parts of a story which are *prima facie* disconnected. In almost any folk story current versions appear jerky, perhaps incoherent in parts, and very badly strung together. The events described are left to be connected by the hearer or reader. In reproductions obtained in a community different from that in which the original version was developed, the observer, acting almost always unwittingly, supplies connecting links. The net result is that, before long, stories are reduced to orderly, humdrum, commonplace narratives. They have been denuded of all the elements which leave the narrator, hearer, or reader puzzled and uneasy, or they have gained specific associative links which, within their own social community, could be safely left to be immediately understood.

Suppose the very same observers are presented, however, with well-ordered argumentative passages, complete with all the associative connexions that could be desired, and are required to make repeated reproductions of these. It does not follow in the least that such bonds of connexion will be accepted and retained. They fulfill their function by making the material apparently coherent. The *form* of whatever is presented may produce its effect, even though the elements of which the form is constructed are but slightly attended to. Any normal, educated observer strives after associative links, but whether the actual mode of connexion or matter of such links, when they are supplied, is faithfully reproduced is another question altogether.

Rationalisation in regard to form found its main expression in the linking together of events within the stories; rationalisation as concerned with the details of material was usually carried out by connecting the given items with something outside the story and supplied by the observer himself. This second case took three main directions.

First there was the process, in all instances witting during its early stages, but later producing unwitting transformations, whereby presented material was connected with other matter outside the story, but having the same general nature. For example, in one story a leading character dies, and in the description of his death

"something black" is said to have issued from his mouth. By a number of observers the "something black" was immediately interpreted as a materialisation of the dying man's soul. The case in which an incident telling how a raven's beak turned into a knife was both accepted and preserved by being treated as a symbol in a murder dream falls into the same category. All such cases had a first stage in which the material rationalised was treated as symbolic. But in the complete process the symbolised material eventually wholly replaced that by which, in the original, it was symbolised. The final step of this replacement was in all cases unwitting. It may well be that all processes of symbolisation fall psychologically into place as subordinate to a wider process of rationalisation, and that a complete process of symbolisation invariably ends in the obliteration of the symbol.

The second process of rationalisation illustrated was unwitting from start to finish. The transformation immediately of "something black" into "foamed at the mouth" was an instance in point. So was the changing of a perfectly irrelevant remark at the end of one of the stories to the effect that "And so the sparrow never got home" into "and so the sparrow got home at last, and here ends my story." No symbolisation, in any strict sense of the word, was involved in this type of rationalisation. At no stage of the transformation had the material employed a double signification, so far, at any rate, as the observer could say.

This is the type of rationalisation in which individual peculiarities come most clearly into play. In the first type a part of the process is witting, and this part always tends to follow the lines of current belief, or of the modes of language expression which have been built into the general communication habits of a community. Thus it is fairly likely to go along the same paths for different members of the same social group. In the third type, as I shall show directly, although the process is unwitting, the results are extremely likely to display the same character throughout a given community. It is in the second type that individual bias and interest most directly determine the transformations effected.

The third type of rationalisation is very closely related to the second. This is the instance in which some particular, and maybe isolated detail is transformed immediately into a more familiar character. In my stories, for example, 'canoe' rapidly became 'boat'; 'paddling' became 'rowing'; a 'peanut' became an 'acorn'; a 'bush-cat' became an ordinary 'cat'; 'Kashim' (a proper name for 'shelter') became 'cabin' and so on. Of all processes of rationalisation this was the most common.

Both the second and the third type of rationalisation are unwitting; neither results in the explicit provision of definite reasons, and both consist in changing the relatively unfamiliar into the relatively familiar. But the second is characteristically individual, so that an incident is likely to be transformed or developed differently as it is dealt with by different observers, while the third type is apt to exhibit the same results so long as the observers are drawn from the same social class. Changes of this type, therefore, which nearly all concern the names of common objects, or special phrases and the like, may be of particular importance in any attempt to trace the line of passage of material from one social group to another.

The general function of rationalisation in all these instances is the same. It is to render material acceptable, understandable, comfortable, straightforward; to rob it of puzzling elements. As such it is a powerful factor in all perceptual and in all reproductive processes. The forms it takes are in many instances directly social in significance. How they are built, if they are to be regarded as "built" at all, it is for other or for more extensive experiments to determine.

4. What Constitutes Outstanding Material?

I will next state very briefly the main types of material that are likely to take a leading position in reproductions under the conditions of the present experiments.

1. There is a strong presumption, most definitely increasing as the observer approximates to a true "vocalising" type, that words or phrases popular at the time of the experiment in the community to which the observer belongs will stand out prominently from their background and be reproduced without change as to form, though possibly with change of position.

2. Any word, or combination of words, or any event which appears comic is almost certain to be reproduced. Nevertheless the comic is extremely liable to change, for what appears comic varies within wide limits from person to person. Proper names, for example, which have a peculiar liability to produce laughter, are excessively likely to be transformed.

3. Material which directly or indirectly stimulates a preformed interest is sure to reappear. Probably the affective tone accompanying the arousal of such interests is an important factor here. This affect certainly is not always pleasing. On the whole the results indicate that if the interesting material is pleasing, the change is in the direction of elaboration and development; if the

affect is displeasing, distortions are most likely to occur. Persistence of material, in these cases, seems to be due rather to the interest or bias evoked than to the feeling itself. But the evidence of other methods ought to be called in at this point.

4. There is a puzzling class of material which appears striking by reason of its "triviality". Much further analysis is doubtless called for in order to determine when and why material is treated as trivial. But when all is said that may be, it still remains that the "trivial" is often the "striking" and as such is likely to be retained.

5. Inventions

No really convincing or valuable psychological treatment of invention has, I think, yet appeared, and I can say but little about this important process on the basis of the present method. Invention, or the importation of genuinely new material from outside into the reproductions was comparatively rare. The evidence seemed to indicate however that as a rule inventions could be traced to a play of visual imagery. Particularly was this the case with inventions which were introduced late in a series. On the whole definite visualisation inspired confidence in the observer. With lapse of time the material was apt to appear relatively dim and blurred. If, at this stage, some associated image of a visual nature occurred, it was apt to stand out clearly and the confidence it produced led to its acceptance as a genuine part of the desired reproduction.

In the second place, undoubtedly certain forms of story definitely favour invention. For example, cumulative stories, of the type of *The Old Woman Who Went to Market,* belong to this class. It thus appears that invention is partially dependent upon the form of the material which is recalled, and partially dependent upon the method of recall adopted. But it is obvious that at this point other modes of experiment must be used and that very much more research is needed.

6. Delay in Manifest Change

I began by pointing out that, as regards their general form and many details, reproductions tend to persist unchanged for long periods. I shall conclude by reference to a characteristic which may appear to be precisely the opposite to this. In a number of cases changes were brought in at a comparatively late stage. They were then often foreshadowed some time before they made their

appearance. For example, an observer who had completed one of his reproductions then casually remarked: "I've a sort of feeling that there was something about a rock, but I can't fit it in." He gave the matter slight consideration and finally rejected the notion. Two months later, without a word of comment or explanation the rock took its place in the story. There was no rock in the original. All the various transformations or inventions that occurred in the course of an observer's reproductions were apt to get connected together, and also, in a number of cases tales reverted to their original form after an interval during which deviations from the original had occurred. Thus the conditions which led to manifest change may be delayed in their operation.

Such delay—and we get numerous illustrations in everyday experience—raises difficult problems. For one thing it seems to mean that when an attempt is made to establish causal relations between psychical processes, mere temporal sequence may be of comparatively little significance. We cannot look with certainty for the leading conditions of a particular response to other reactions immediately preceding the one which we are trying to explain.

A particular stimulus, or feature of a situation, gives rise to a tendency to respond in a specific manner. At first the tendency is held in check and produces slight or perhaps no manifest results. As time elapses apparently the unexpressed tendency may gain strength and so manifestly affect the response, or other tendencies simultaneously excited may lose strength, and in this way also a new manifest change of response may appear.

Can we understand how an unexpressed tendency may gain strength? There appears to be little real ground for holding, as some do, that the mere checking, suppression, or damming up of any tendency is able to add strength to that tendency. But perhaps we may hold that a weaker tendency may gain in strength by being associated with a stronger one. Many of the manifest changes, when they appeared, came in close relation to other transformations which were actually made earlier in the series of reproductions. Perhaps some of the delay in manifest change is due to the linking together of tendencies which are initially weak to those which are stronger than themselves. But there is little genuine evidence as to how this linking process takes place.

On the other hand, such transformations as that of the introduction of rocks into a story several months after its first reproduction, and two months after rocks were vaguely attributed to the tale, seems rather to be a case of the weakening of certain tendencies and a consequent relative strengthening of others. For the

manifest change, when it appears, does not seem to be specifically connected with any preceding change. Whether, and in what precise sense, this view can be maintained must depend upon definite experimental evidence as to the effect upon different and particularly upon competing tendencies of the lapse of time. For on the face of it there is little or no more reason for assuming that tendencies weaken with lapse of time than that they are strengthened by being merely denied immediate expression.

Finally, the whole notion of an unexpressed tendency, continuing for long periods to have the capacity of coming into operation, while to all outward appearance and to inward observation also it is wholly in abeyance, is not easy to understand. The notion seems to be demanded by many of the facts of mental life, but it clearly calls for a very critical consideration.

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THE DEVELOPMENT AND FUNCTION OF CLOTHING

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The psychological problem of clothing is a complicated one. The relation of clothing to ornament, the actual functions of each in the social and individual life of man and woman, the relation of each to modesty and salacity, their hygienic relations, and their effects on mental development and efficiency; all these are of grave importance. To a certain extent these problems are independent of the problem of the origin and past developments of clothing and adornment. Yet, to an important degree, the understanding of present situations depends on the understanding of origins. For this reason, I suppose, speculations concerning the origins of clothing have been rife from the most remote historical times.[1]

I think it is fairly accurate to say that there have been but four important theories as to the origins of clothing. These are:

1. *The modesty theory.* This theory, familiar from the Mesopotamian legend of the Garden of Eden and Eve's seduction by the serpent, holds that clothing was originally donned to conceal the genital organs, from a sense of shame, modesty, embarrassment, or some other form of sexual emotion. From this beginning, it is assumed, has grown the practice of covering the body more generally, as sexual self-consciousness has become more refined.

2. *The immodesty theory.* This theory, popularized by Westermarck and Havelock Ellis, maintains that the intent and purpose of clothing in the beginning was salacious, designed to attract attention to sexual organs and sexual functions, and in general to make the wearer a greater object of sexual interest. This is the doctrine that familiarity breeds indifference, and that concealment, especially pretended or partial concealment, increases interest.

3. *The adornment theory.* The theory that clothing begins in the desire to attract attention, or secure preëminence, not necessarily of a direct sexual sort. The primitive clothing on this theory is conspicuous ornamentation.

4. *The utility, or protection, theory.* The supposition that clothing had its origin in attempts to protect the body from injurious or unpleasant features of the environment would seem to

[1] As an introduction to the psychological study of this topic, and for references to earlier literature, the excellent paper by Professor Sanborn on The Function of Clothing and of Bodily Adornment, *Amer. J. Psychol.*, 38, 1927, 1-20, is to be recommended. It will be clear to the reader that the present author does not agree with Professor Sanborn's conclusions.

be the most obvious one, but for various reasons has had little standing among the anthropological theorists.

There are, of course, minor hypotheses, covering specific details of clothing development, but the four theories listed above are the main ones. The adherents of each theory admit that the other theories account for some of the features of clothing, but deny them as explanations of the primary motive in dress. The modesty theory has probably had the greatest vogue in modern civilization, through its having crept into popular religion, and so having been connected with moral prejudices. Eve and her fig-leaves still trouble popular moralists and bob up in police regulations.

It may sound merely facetious, but it is both metaphorically and literally true to say that every time woman lifts her skirts the moralists shiver. Yet this is not the only way in which she has frightened them with her clothes. Some years ago, shoulder puffs, and baggy sleeves, were denounced as indecent and ungodly. The shudders over the one-piece bathing suit have not yet subsided, in spite of the valiant missionary work of Mack Sennett. That bobbing the hair flaunted indecency, threatened the foundations of morality, and endangered the sanctity of the home, even the College Freshman can remember. He probably does not know, however, that not many years ago, putting up the hair on top of the head was also looked upon with fear and indignation.

Any considerable change in woman's attire arouses terror among the popular moralists, but this effect is not casual or accidental. It is based on the theory that morality depends on modesty, and that modesty is the concealing of the human body, especially the woman's body. The moralists being mainly male, and hence very much interested in woman sexually, it is natural that the greater emphasis in modesty should be on the modesty of woman.

This modesty theory of clothing seems still to be firmly intrenched among the ignorant, in spite of the various lines of evidence against it. Its falsity should have been evident long ago from the fact that oriental women have had to conceal their faces to be modest, while occidental women have not been under similar necessity; and the fact that naked savages are often more modest, (and more moral too, for that matter), than clothed Europeans. That women are as modest now in knee-length skimpiness as they ever were in hoop skirts or in sidewalk-sweepers, should be apparent even to those whose knowledge extends no farther than their village boundaries. But the sad fact is that attempts are still being made to regulate woman's attire in the alleged interests of morals, and the more timid males are still in dread of what woman may leave off next. The obvious failures of regulatory

measures in the past, and the equally obvious certainty that if women decide to discard clothing entirely they will do so, in spite of selectmen's ordinances and the protests of clergy and press, seem to convey no lesson to the weaker brethren.

The second theory, that clothing was introduced primarily for the sake of its immodesty, is based on the observation that the covering of the sex organs, by people previously accustomed to nudity, has a salacious effect, and promotes indecency and immorality. This is at least as plausible as the immodesty theory, but just as improbable, since there is no actual evidence that the custom of wearing clothing has arisen in either way. It is no doubt true that the debauchery of naked savages by Europeans has in many cases been assisted by compelling or inducing the savages to wear clothing of European types; but this clothing had already been developed by the Europeans, and it can not be shown to have been developed for the sake of such effects as it produces on the savage. The study of ancient and savage social conditions and clothing habits produces nothing but evidence against both the modesty and the immodesty theories.

As a matter of observable fact, the connection between clothing and modesty is a simple one. Any degree of clothing, including complete nudity, is perfectly modest as soon as we become thoroughly accustomed to it. Conversely, any change in clothing, suddenly effected, may be immodest if it is of such a nature as to be conspicuous. This is particularly true for men of limited intelligence and low education. The more highly cultured and educated individuals adjust themselves to new conditions with less shock. Clothing itself has no modesty or immodesty. It is merely the breaking of the established convention which makes it immodest.

The difficulty which has been found in facing these facts is really due to the difficulty which has been found in explaining how clothing came into existence. We assume that the wearing of clothes in the first place has a cause, that it is not merely accident; and the modesty theory has seemed a plausible explanation because no other theory seemed more plausible. The wide and persistent acceptance of the fig-leaf theory can be given no other explanation. In a similar way, the immodesty theory has been adopted by a few who have seen that the fig-leaf theory is impossible, and could see no other way of accounting for clothes.

The first and second theories go down together. There is today no reasonable doubt that clothing has nothing intrinsically to do with modesty or immodesty, although changes in costume, in either direction, may have salacious effects. Either clothing or naked-

ness may be worn modestly or immodestly. The only modesty of clothing is its wontedness; the only immodesty, the suggestion that it might be removed. We must look elsewhere than in sex feeling for the origin of attire.

The third theory, the theory of adornment, is perplexing. It involves the curious assumption that adornment is self-explanatory. The theorists offer more or less vague amplifications of the conception of adornment, in terms of "extension of personality", "aggrandizement", and so on; but when their phraseology is sifted down to matter-of-fact statements, they have really nothing to offer except the assumption that clothing is worn to make the wearer conspicuous. We may grant the human tendency to seek the lime light, the desire for preëminence; and we may grant the influence of this in the development of clothing and adornment. But that conspicuousness is attained by adornment when every member of the group wears the same attire or ornaments, can not be granted. If the simple desire for conspicuousness were operating, the individual who should omit the ornament would be the most successful. He would in fact be the most conspicuous; but he doesn't do it. It is reasonable to conclude that there is a reason back of the adorning, that ornament is not a simple "instinctive" product, requiring no explanation, but that it requires to be explained along with clothing. Granted a clothing system, ornament may be expected to enter into it, both in the factor of conspicuousness, and less frequently in the esthetic factor. But antecedently, there is another factor, which we must discover. Let us therefore pass by the adornment theory for the moment and consider the fourth theory of attire.

This theory, which maintains that clothing was introduced primarily for purposes of protection, has been in disfavor, because it has not been easy to see how the most typical primitive clothing really affords any protection. It has been freely admitted that the costume of the Esquimaux protects from cold, and has been developed for that purpose. It has also been admitted that the heavy skin cloaks or karosses of other peoples are worn to protect from rain and cold. Sandals, leggings, arm bandages, and similar paraphernalia are undoubtedly developed as protections from stones, thorns and brambles. But these are really minor details in the development of dress. For most savages, and probably for primitive man, protection against cold is only an occasional matter, and many races make no efforts to protect themselves by clothing, even though inhabiting climates where cold is encountered; and yet these races develop clothing of a characteristic sort. Sandals, leggings, and arm protectors, too, are incidental, put on and discarded

in conformity with the occupations of the savages. Costume exists, quite aside from these forms of protection, and apparently was developed before these.

The most characteristic types of costume of savage races, types which are believed to be truly primitive, are really represented by the fig-leaves of the Babylonian legend. Skirts and aprons of grass and leaves, and bunches of leaves hung at other points of the body are really worn by savages in various parts of the world. Similar dangling garments made from the skins of animals, or their tails, and sketchy garments woven from vegetable fibers, are also widely popular among savages. These garments give no protection from cold, neither do they protect from thorns and brambles, or to any great extent from the sun. Even where loin-cloths and sarongs are worn the protective factor is not obvious. Why, then, have these types of clothing been so widely and primitively developed, if not for concealment from the eyes of other people? Or else to emphasize the sexual parts of the body. The fact that the peoples who wear these garments put no emphasis on concealment, but at times discard the garments completely; and the fact that where there is a costume which can not be discarded in public, it is apt to be some non-concealing article such as a string of beads around the waist, dispose of these explanations, and apparently leave nothing but the unsatisfactory adornment theory in the field.

As a matter of fact, these garments of the fig-leaf type are really protective, and our failure to recognize this has come from our incomplete analysis of savage life and its conditions. Savages really need little protection from cold. The American Indian and the Patagonians demonstrated this very well. In warmer climates this sort of protection is almost nil. Protection from other features of inanimate nature is an occasional need. But man has a group of adversaries from whom protection is an acute need, especially in the warmer climates in which man undoubtedly originated, and in which large groups of savages live today.

We are becoming familiar with the notion that the great battle which man has to fight, and which he has not yet won, is against insects.[2] Insects destroy our food supplies, devour our buildings and infect us with deadly diseases. The ant, the mosquito, the termite, the house fly, the Hessian fly, the gypsy moth and a host of others, menace us constantly. Modern conveniences, however, have largely freed us from the plague of insects as sources of immediate personal discomfort, and we are apt to ignore the fact that for the savage, and for primitive man, the situation is quite

[2]The term "insect" is here used in the comprehensive popular sense.

different. Flies, mosquitoes, lice, fleas and other crawling and flying pests are with him abundantly, and very intimately. The African savages grease themselves copiously, and grease their skin cloaks, as a protection against insect pests. Yet this protection is not complete. After they have inhabited huts for a time, they are forced to remove and build new ones, leaving the old dwellings to the increasing swarms of cooties and their coadjutors. Among the early Mediterranean peoples, the removal of the hair from the body seems to have been a measure to reduce the entrenchments of these enemies. These expedients avail somewhat against the crawling enemies, but not against the flying ones. Flies, however, are especially persistent and irritating in their attacks on the moist and sensitive membranes of the eyes and the external genitals, and some of them bite right through the skin anywhere.

Now, what sort of protection is most adequate against flies and similar insects? Wrapping skins or cloth tightly around the body is successful, but to persons unaccustomed to such unhygienic clothing, especially in warm climates, this method is intolerable. Much more efficient protection is afforded by hanging strings, leaves, strips of hide, animal's tails, and similar articles so that they will flap with the movements of the wearer. In other words, the best fly chasers are exactly the garments most characteristic of savages and primitive man. These afford protection without undue warmth or exclusion of ventilation.

Confirmation of these statements lies close at home. The fly-net, much employed in the days of the horse and buggy, was a contrivance of loose and flapping strings thrown over the harness. Tassels attached to the bridle near the horse's ears served a similar function. An expedient much employed on farms was the attachment of bundles of leaves to the harness in appropriate places. I have seen work-horses so equipped as late as last summer. An apron of burlap or cloth is sometimes bestowed on cows in fly-time. In short, the fly-protections we have customarily used on our domestic animals are exactly of the types of primitive human clothing which have baffled the early anthropologists.

The origin of clothing, properly so called, can then be ascribed to the human need for protection from injurious and unpleasant agencies, although other factors have entered into its further development. There remain, however, ornaments of various sorts which still require explanation—ear rings; nose rings; labrets; arm bands and leg bands of furs and wire; necklaces and girdles of teeth, seeds, claws, and later beads; tattooings and scarifications made by gashing the skin; these give no protection whatever, although the armlets and leglets are patterned after more practical

wrappings of skin or cloth worn for protection in the field or jungle. These savage adornments, however, serve practical purposes in the beginning; purposes as practical as the protection afforded by clothing.

Among savages, every detail of social status is apt to be represented by a badge of some kind. When the boy or the girl is initiated into manhood or womanhood, a mutilation, or some other badge, is conferred. At marriage, another badge is bestowed. When a warrior kills his man, he adds some ornament to his body, or cuts a gash on the body of his wife. (The American 'bad man' cuts a notch in his knife or revolver handle.) And so on through a long list of social distinctions. Some of these mutilations had other practical values, for example: circumcision of the male to increase the sexual stimulation of the female; knocking out teeth so that artificial feeding could be employed in case of lock-jaw; but even these mutilations had their important practical uses as badges of social status. From the ear rings, nose rings, haircut, headdress, tattooings, scars and other badges of the savage in full regalia, the expert can tell at a glance to what tribe the savage belongs, whether he is a warrior or counselor, whether married, and if so, the number of his wives, how many sons he has, and whether these are warriors or not, how many cows he owns, and so on throughout the savage's complete Bradstreet's and social register.

When we include among practical factors not only protection from the elements, from brambles, rocks, rough grounds, reptiles, and insects and various other incidental factors of the environment; but also the eminently practical factor of badges, we have accounted for the origin of the entire repertory of savage and primitive clothing and ornament. The development of attire from that stage is largely a matter of habit. What are at first important as badges, becoming customary, and being desirable on account of the social indications, become later desirable from mere convention, that is, as pure ornaments. The diamond necklace is today desirable partly as an actual indication of wealth, (and therefore of social status); partly because it earlier indicated social status which wealth alone does not give. High-heeled shoes, originally the badge of the lady or the courtesan, indicating her fortunate position of exemption from work, became "stylish", and working women today deform their feet and acquire a cow-like gait because of this earlier significance of high heels. Various other types of clothing developed for protection, or as badges, become conventional and are clung to ages after the practical factor has vanished.

The association of clothing and modesty developed slowly. The Greek girl cast off her garments in public when it became incon-

venient. Even in the Middle Ages in Europe, when women wore skirts but no underwear, the incidental exposure of the genitals had no specific flavor of indecency. The savage discards his aprons or loin cloth when it is not needed, with no sense of shame, and with no effect on the sexual sensitivity of his fellows. In Japan, men and women appear naked in public without any effect on modesty, although ordinarily fully draped. The modesty of clothing seems to have appeared only when through the invention of elaborate and costly materials and expensive dyes, it became possible to display one's wealth and indicate one's social position by the mere quality of valuable clothing worn. Then began the practice of completely enveloping the person in expensive cloths, laces, velvets, feathers and gold work. Clothing as a badge made the concealment of the person habitual for the upper classes, although not until much later for the lower and poorer classes. It must not be forgotten that among certain races there is a similar modesty of ornament, and that among certain African tribes a woman without her significant string of beads around her waist, (although she might wear nothing else), would be as ashamed and dishonored as would be a European woman caught stark naked.

From these early stages, the development of male and female clothing is different in various parts of the world. In Europe, the inconvenience of flowing garments led men to the adoption of knickers and kilts, and the inconvenience of these, to trousers as the standard male uniform. Woman's attire lasted longer in its more absurd stages, both because the practical inconvenience was not so great for woman, and because woman will endure more discomfort and inconvenience in order to appear important than man will. The expansion of manufacture, trade, and invention, however, eventually cheapened woman's attire, and it became possible for the working woman to appear in silks, laces, and other fripperies which were as showy, if not as expensive, as those of the chatelaine or the courtesan. It was inevitable, therefore, that the expansiveness of woman's clothes should be doomed, and that like man's they should become practical. A generation ago any one could distinguish the prostitute from the working woman on the street by the clothes she wore. An expert could distinguish her from the lady by the way she wore them. Then the working women began to imitate the prostitute's apparel, and the distinction became more difficult. The invention of anniline dyes, synthetic gems, and later, artificial silk, have been the most powerful causes of the shortening and general improvement in women's clothes. Furs remain the only sartorial indications of wealth and social status which can not be cheaply produced or imitated. Hence the persistence among

women and effeminate men of the delight in the skins of dead animals.

During the period in which wealth and social status could be indicated by the amount of clothing worn, the habit of concealing women's legs became established. Hence, with the beginning of modern styles, the exposure of the ankle was indecent. That is to say, the ankle having been hidden from view, its exposure attracted attention, and of course strongly suggested other, still concealed portions of the anatomy. With each increasing public revelation of the leg, the same salacity was introduced, but with lessening effect. We become accustomed to the conventional exposure, whatever it is, and with a series of changes we become accustomed to the fact of change itself. Hence, the exposure of the thigh will occasion only a minor commotion, as compared with that which was produced by the exposure of the ankle. I have no doubt that within a few years women will expose the entire body in public, with negligible effect on modesty and morals, unless for the better, for there is no denying that although neither clothing nor nudity have any intrinsic value for modesty, clothing has the greater potentiality for immodesty, since it has the power of suggesting what is concealed.

We have now put ourselves under the necessity of answering a new question. If clothing is not conducive to modesty or to morality, but rather the contrary, why has it not disappeared? Its value as a badge has become minimized. Its protective value is not important except under conditions which do not obtain much of the time among civilized peoples. It is not conducive to health. Man could profit in comfort, and hygienically, by reducing his winter attire as woman has reduced hers, and in warm weather both sexes would be better off, hygienically, if they completely discarded raiment the greater part of the time. But still we cling to clothes as the oyster to his shell, and the police back up public opinion. Is this mere force of habit? Habit of course plays a major part, but there is another reason why we conceal our figures; a reason of vital importance to racial betterment; a reason so strong that it is possible that women may still return to long skirts and pads, and man may never emerge from his swaddlings.

The reason is to be found in sexual selection and sexual competition. We may doubt the eugenic value of sexual selection but it is nevertheless an important phenomenon in all grades of society, and profoundly influences social and individual life. Sexual competition is based on sexual selection, and is perhaps the factor of greatest importance in the preservation of certain clothing habits, once they are established.

The principles of selection are different for the male and the female, and I think the effects of this difference are recognizable in the sartorial developments of modern times. The female is evaluated primarily on the basis of what we call "beauty", which involves certain objective standards.[3] These standards apply to (1) form and proportion of body, (2) coloration of skin, hair, and eyes, (3) nutrition and general health, (4), coordination in movement and in posture, and (5) certain minor factors such as texture of hair and skin, form of eye, mouth and other features, quality of voice, intelligence and temperament. Other standards exist also; standards of accomplishments, of social caste, and economic circumstances, but beauty is by far the most important consideration.

Standards of beauty vary from race to race and from time to time. This is especially true of the fifth group, but applies to the others also. Standardization, however, is always on a practical basis, although the actual standards, once established tend to outlast the practical conditions on which they are based. Where no specific practical determinants exist, the characteristics peculiar to the race are important, the Chinaman preferring the normal Chinese type, the Senegambian the normal Senegambian type. The practical factors are frequently more important than they seem to be to superficial observation. Where food supplies are precarious, and the reserve fat which the woman may accumulate for the benefit of the child is of great value, preference for the obese type may be expected, as among certain African tribes of worshippers at the shrine of Aphrodite Steatopygia. Purely casual factors may from time to time become important, such as the taste for red hair which flourished for a time, supported perhaps by the rhapsodies of certain poets whose inamorata happened to be adorned with sunset hued locks. Prevailing fads have their effects, since man wants his women to be approved, if not envied, by other women. Hence the contemporary feminine penchant for pathological thinness affects temporarily the preferences of the male, although his fundamental standards are probably unchanged, and he would be relieved by the return to more normal proportions. However important may be the variable factors in beauty, the fundamental anatomical and physiological characters always have been preëminent, and for the benefit of the race we may well hope that they always will be.

Standards of sexual attractiveness of the male are characteristi-

[3]A more detailed discussion of these standards and their importance is given in the author's small volume on *Personal Beauty and Racial Betterment.*

cally different from those of the female, but are likewise on a practical basis. Certain desiderata are definitely established among Western races (although we do not refer to them as features of "beauty"), and corresponding ones obtain among other races. These are: (1) stature above the average, (2) muscular development for both strength and agility, (3) lung development for endurance, (4) racially normal features, (5) thick hair, preferably curly, and (6) aggressiveness and economic ability. Here again, cultural conditions modify the standards, and fads have their influence, but these modifications are less noticeable than in the case of the standards of female beauty. Economic ability is of course variable in form. It may be hunting and fighting ability, or ability as a herdsman or trader, or in the mechanical arts, or in politics or banditry.[4]

Competition on an open and honest basis is not satisfactory to the majority of competitors when these include a wide range of abilities, since only a few will rank high in the competitive characters. This is markedly true in sexual rivalry. It is evident that labor union rules reducing competition by limiting the amount of work per day to a low maximum, and establishing a flat rate of compensation, operate chiefly to the advantage of the less competent workmen. In sexual rivalry a similar result is brought about. If we consider but one character of sexual importance in both men and women, namely, the shape of the legs, we can easily satisfy ourselves at any popular bathing beach that only a small percentage for either sex are presentable. So far as women are concerned, short skirts have revealed the hideous truth, (or the hideous legs), on the street. In lesser measure, the funny clothes which golfers and college youths wear on their nether limbs have shown up the inadequacies of male shins and calves.

A few years ago, competition in women's legs was non-existent, except for positions on the stage. Now, it is open and impressive. It is no accident that the opening up of this competition has been bitterly fought every inch of the way, by women of certain types, as well as by moralist males. The competition has been forced, however by the group of women who have been confident that they would not suffer in open comparison, and the others have fallen resentfully in line. The powers urging a return to longer and more voluminous skirts are not made up exclusively of cotton and wool growers and manufacturers.

The solicitous hiding of the legs of the male is due in part to

[4]The bandit has apparently been the economic type preferred by women through the ages. He is the first, and perhaps the only real "gentleman."

the fact that trousers are more comfortable and convenient than either knickers or the comical chauffeur breeches we wore during the war. But this is only a part of the story, for short pants are more comfortable than trousers for many occasions, and yet men are disinclined to adopt them. This reluctance, I am sure, is a wise one for the majority of us. Hygienic and comfortable garments for men are still a considerable distance remote in the future.

An objection might be made at this point by calling attention to the fact that relatively little fuss has been made in the past over the exposing of women's arms, and that competitive exposure in this respect has been long wide open. This fact is, however, distinctly in favor of my position, since there is really little competition in regard to female arms. The arms of the woman are individually and collectively of little sexual interest to the average man, even when concealed, and the variations in these members do not show the distinctive graduations shown by the legs. A few women, a negligible few, have arms that might be considered beautiful. The rest are neutral, falling into a group in which the differences are of no consequence. It is probably just as well for the Venus of Milo that she lost her flippers. They could hardly have added anything to the ensemble, and might have detracted.

Concerning the arms of the male there is a different story to tell. Here the muscular development of the members is a character of importance, for hunting, fighting, agriculture and mechanics. The effects of these ancient capacities still persists. Moreover the male embraces the female, and aside from any associated ideas, the strength of the embrace is appreciated. It is therefore quite in accordance with our point of view that man should cherish his heavy coat sleeves, and should frown upon shirt sleeves, (for these reveal deficiencies somewhat), and bare arms, where these can well be avoided. The majority do not welcome the devastating competition of the favored few. The insufficient biceps and pusillanimous triceps are not so much of a detriment when heavily camouflaged.

The hiding of the face by the women of the Near East almost completely abolished sexual competition, except by the circuitous method of match makers and go-betweens. The figure being voluminously draped, the facial concealment left everything to the imagination except the eyes, and liberal applications of cosmetics uniformized those. It is evident, however, from Arabian literature, that the veiled and shrouded women excited no dearth of salacious imagination. The breaking down of this almost complete trade-unionism in restrain of competition constitutes an important psychological event, of which we know only meager details. Apparently the union rule was broken down by certain

aggressive women who felt that they had nothing to fear from competition. If some could expose their faces and get away with it, others would be compelled to follow suit, or else be exposed to extremely invidious speculations as to the impulchritude of the features they concealed. In the same way, if the possessors of the more shapely legs expose them, those afflicted with the more unfortunate limbs must expose them also, or else be suspected of being more unfortunate than perhaps they really are.

It is a noteworth fact that as the competition in figures opened up in Western lands, women here began to cover their faces with masks of paint and powder, so that the earlier specific significance of the term "painted woman" was completely lost. It would seem that as one union rule in restraint of competition was broken down, another compensating restriction was set up. A mask, like other disguises and disfigurements, reduces competitors to a more equal basis. The advantages of the fresh and blooming complexion, (a natural index of other features of sexual attractiveness), as compared with the sallow, spotted and leathery maps of older or less favored females, are lost when all are hidden by the enameling and calcimining process. It is possible that the proponents of the short and skimpy skirt would not have won so easily if the defeated majority had not been allowed to compensate for this loss by a facial gain.

Looking back over the history of clothing and adornment, we find a great deal which has been effective in equalizing sexual competition, reducing the competition to a less deadly level. Gloves make the hands of washerwomen and princesses look more nearly alike. Long hair permits deceitful coiffures which tend towards the uniformization of the luxuriant and the scanty locked. Hoops and bustles make Venus and the Harpy alike, below the belt. The corset squeezed all waists into an indifferent ugliness. Even long skirts, without crinoline, reduce the limbs of the goddess to somewhat the level of the knock-kneed, the bow-legged, the beanpoles, and the parlor grands. Plucking of eye-brows is another great leveler. Paint and powder, henna and kohl, parasols and fans, are among the effective union weapons in restraint of competition.

The dominant male also has not been without means and methods for the neutralization of natural defects and advantages. The camouflage of coats and trousers has already been mentioned. Robes, and other flowing and flossy garments were early worn by men of superior social status. These garments concealed not only bandy-legs and bull-calves, but also emaciated figures and aldermanic abdomens, and were undoubtedly great aids in preventing

the women of the upper class men from sighing after more Adonis-like males of the exposed lower classes. Beards have been natural concealers of faces handsome and otherwise among many races. The abolition of the beard among modern men of Western Europe and America has been of course largely due to the influence of woman, who appreciates the smooth face in personal contacts. It is not probable, however, that woman would have succeeded so soon in separating man from his natural growth of facial hair if this growth had not become so diversified that it constituted a distinct feature of individual differences. Too many men today would have beards of painful scantiness, peculiar color, or insufficient distribution on the face, if they should let them grow, to bear comparison with the well-bearded man. The beard actually became a competitive point instead of a means of concealment, before it was abolished.

The story of clothing is now practically complete. Having its primitive origin in practical protective needs, its amplification and retention under conditions which render it practically unnecessary, has been fostered by the resistance to sexual competition which is today the strongest force operating against dress reform for both sexes. Bobbed hair, the short skirt, and the peekaboo waist may be denounced in the names of sexual modesty and sexual morality; but their real objectionableness lies in their furtherance of sexual competition. The attack on the one-piece bathing suit may have been headed by male moralists, but its energy was supplied by women who had good reasons to distrust their ratings in a bathing beauty contest.[5]

In some respects, at least, savages have been wiser than civilized peoples. Both physical and mental hygiene demand clothing that shall not transgress the limits of protective needs. In general, we wear too much clothing, and of the wrong kind. Woman, long the worst offender, has suddenly outstripped man, both literally and in the line of progress. Man does not need the protection

[5]The aversion to bodily competition on the part of women is complicated by the fact that she fears the criticism of her competitors more than she fears the estimation of men. It is generally known that women dress for the eyes of other women, rather than for the eyes of men, who, in general do not notice the details of female attire, being much more interested in woman herself than in her clothing. It is a fact also, that most women, regardless of their pulchritude or impulchritude, would rather unveil themselves completely before men than before their fellow women. They fear the criticism of other women on precisely the points of their sexual appeal to men. The corresponding inhibitions of men in respect to their own sex are far slighter, as shown by observation of their general behavior, and by the radically different conditions in gymnasiums and swimming pools for the two sexes.

from cold which he claims, in winter, and in summer he needs protection only from flies, mosquitoes, and the rays of the sun in certain climates. Progress is retarded by a professed moral censorship that is really vicious. Every activity in this direction has proved itself ridiculous and obstructive. The restraint of sexual competition may be useful in certain stages of society, but this usefulness, if it occurs, soon disappears. When we shall have returned to the primitive basis of clothing, as a means of protection and nothing more, we will have lost most of our problems of sexual morality, and sexual immodesty will have disappeared along with its reflection, sexual modesty.

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GENERAL LAWS OF REDINTEGRATION

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In a redintegrative sequence, as the terms are now used, a group of co-stimuli constitute the antecedent of a co-response or consequent, which is usually complex. This account differs from the "conditioned reflex" hypothesis in various ways. We may overlook certain obvious points that have often been noted, such as that the so-called "reflexes" of this theory are not reflex, that the response is not really "transferred", that repetition is often not required, and so on. The fundamental difference is one of another sort.

The conditioned reaction hypothesis describes only one half of the simplest possible mental activity. It ignores the fact that the secondary stimulus has its own response, which it continues to evoke even after the "conditioning". It also overlooks the fact that the "primary" stimulus becomes conditioned to this response, and that wherever one "conditioned reaction" is described there is necessarily another that is ignored in the same situation. Since this is the case, the fact is that either stimulus now evokes *both* responses,—that is, the total co-response formerly made to the co-stimuli. But this is not conditioning,—it is redintegration unless it is only half described. The conditioned reaction is therefore but half of a description of what actually takes place.

We wish here to elaborate the redintegrative process, so as to indicate that this is not merely a convenient word. There are many redintegrative laws and sub-laws, not one only. Waiving, for the moment, numerous special laws, we here consider an array of general laws of the redintegrative sequence.

The Factor of Complexity

It is necessary first to consider complexity and identity. Observe for example the "fear" that ensues upon the "appearance of a snake", and note the numerous details of antecedent and consequent. In the first place the "appearance of the snake" is complex. The snake itself is an elaborate pattern of occurrences,—color, shape, size, movements. Moreover the "appearance" implies the presence of a further background from which this pattern emerges. And there are adjacent objects, the time, place and other attendant circumstances. The fear is not of the snake but of the total situation and circumstances.

The fear which ensues is also a complex pattern of events. For

one thing there is a unique set of sensory qualities,—tension, chill, palpitation, dizziness, and varied quivers. There is an abrupt halt in current activities. Excitement, confusion, panic may occur, and such actions as screaming, blanching, trembling, fleeing or fainting. The situation is colored by disagreeableness, and perhaps also by such feelings as shame and despair. Some of these events are reported in common by the actor and by other observers, —as screaming, trembling, running. Other features can be directly observed and reported only by the fearful,—as the dizziness, the shame, the chill, tension and unpleasantness.

The Nature of Identity

In this connection a purely linguistic difficulty must be dealt with, with sufficient clearness so that it need not obtrude itself into later discussion nor compel constant pedantry and circumlocution. It is of course true that the particular fear just described can never actually *recur*. Fears that have passed are never resurrected, —they are gone forever, just as are former wars and earthquakes. It is always a *new* earthquake, *another* war; so also the later fears are *different* ones, although we may call them the same fears.

Identity is only persistence in or membership in a class. A class is a temporally extended event or series which we name. Hereafter therefore when we say the same event, the same stimulus, the same response, it must be understood that we are not implying that events have ghosts which reappear in Nature. For us "the same event" can mean only "another event in a given class"; and a class is a series that is not fully constituted until all the particulars comprising it have occurred. We thus follow the convenient linguistic custom which calls two events "the same" if they are, for a given purpose, indiscriminable, or if one can be substituted for the other without changing the practical character of the result. It is necessary only to avoid the popular interpretations of such linguistic usage, which have in times past misled not only poets and priests but logicians and philosophers as well.

Selection of a Concrete Example

We wish now to present some of the more general laws of redintegrative activity, each concretely illustrated. For this we need not a careful and quantitatively determined picture, but a genuine one, which is familiar and intelligible. We choose, as the general field for our example, certain of the activities of a neurotic soldier, especially those events which we call his trembling, stam-

mering and running away. The particular soldier we here have in mind was named S.

In line of battle S fell into a terrible panic of fright on the occasion of a violent shell bombardment. He showed external signs from which we infer feelings roughly called fear, and general behavior which we also call fear. His teeth chattered, his knees shook, he started to run with no apparent objective, uttering intermittent and stuttering sounds. Then he collapsed in a heap and showed no gross bodily activity for several hours. Taken to the field hospital, he recovered from his stuporous condition, but was agitated, restless, moved frantically about, stuttered violently when he spoke. Any sudden event served to increase this picture of disorganization, which had first appeared on the field of battle. The sound of bursting shells would throw him again into a state of collapse.

He was moved further back from the front, then to hospitals near the point of embarkation for home. He appeared gradually to improve, but remained so unfit for military service that he was finally sent to a hospital for chronic cases, in his own country, far from the territory of the war activities. Here he was able to get about without personal assistance and made himself contented in the wards and about the grounds. At this point our observation of him, for the present purpose, may begin, in the light of the history here recorded. His activities illustrate most of the general laws of redintegration in a somewhat exaggerated way, much as the microscope magnifies the structures which the biologist observes. But there is nothing novel nor artificial about the picture, —no features that, in somewhat less dramatic form, are not characteristic of redintegrative sequences everywhere in nature.

Effectiveness of Partial Stimuli

The first fact of prime importance for psychology is that *consequents (responses) may follow fragments of partial details of their former antecedents (stimuli)*. So long as S was not brought into contact with rapidly changing events, his activities seemed ordinary. But certain events would, if he were in their proximity, speedily touch off a characteristic pattern of activity, including either stammering, trembling or running aimlessly away, or some combination of these. A sudden sound, a slap on the back, the passing of an officer in uniform, a dog fight, or the handling of weapons or knives,—any one of these was commonly followed by a mild panic in which trembling, stammering or running might appear. A response follows a detail of its former antecedent.

Completeness of Response and Stimulus

A second principle is that *the completeness of a redintegrated response may vary with the completeness of the stimulus.* Consequents aroused by partial details of their former antecedents are likely to be incomplete. Some of their elements may be missing, those present may not arise so promptly, and they may be less intense or less prolonged. That is to say a response to a partial detail of a former context is likely also to be a partial response. But it is a response which is similar to the original and, for purposes of description and often for practical life, is "the same" response,—that is, one for which a given class name is appropriate.

The neurotic responses of S were by no means so violent nor so persistent as was the response to the original experience on the battle field. Some of the elements were often not exhibited. The response was particularly violent and complex if two effective stimuli occurred together, or if one, as a sound, were exceptionally sudden or intense. He once encountered an officer in uniform on a sidewalk, separated from adjacent fields by a muddy ditch. At that moment a motor-cycle engine near by began suddenly to pop loudly. S, who had tremulously begun to reply to the officer's query, now shook violently, began to stammer explosively, then suddenly sprang from the sidewalk, floundered through the ditch and ran unsteadily out into the empty field. The completeness of the response varies with the completeness of the stimulus.

Prepotent Details

A third observation is that *some of the details of the antecedent are prepotent.* They are more effective than others; are more likely to redintegrate the response formerly made to the total situation. These we may call prepotent details; they have what we may designate *greater instigative potency.* What the prepotent details are, in any given situation, or in a given general field, is a matter for special inquiry in each case.

S was not equally easily affected by all of the details to which he was in general susceptible. The stimulating episodes were not all of equal stimulating power. Noises were most effective; a sudden push or slap came next; weakest of all were such visual events as the officer and the weapons. It may be that the noise of exploding shells was the most prominent or persistent feature of the original experience. At any rate, some details are prepotent, others less effective.

Special Determinants of Potency

A given detail may vary in instigative potency, depending on its original intensity or prominence; on its temporal position in the stimulating series; on the various other contexts in which it has occurred in the past; on the recency of the original sequence; on the frequency of its encounter; and especially it may depend on the redintegrative tendencies of other details now occurring with or near it. There are numerous other conditions or determinants of detail potency, which again are matters for careful investigation in every field.

We have already seen that sounds appeared to have their special potency with S in part because of their prominence in the original situation. This may also have been accentuated by the frequency with which such stimuli were encountered even in civilian life, and especially about a military hospital, with its numerous calls, signals and salutes. Gradually, in part merely through lapse of time, S became less and less sensitive to all these influences.

Synergy and Inhibition

Stimulating details from the same or different contexts may work together, thus reinforcing each other (synergy); or they may be opposed in direction and tend to neutralize each other (inhibition). When different events would lead to similar consequences or to responses that do not mutually interfere, they are called consistent, harmonious, congruent or compatible. When they would lead separately to different and interfering responses, they would be called inconsistent, contradictory or incompatible.

We have seen how clearly in the case of S the occurrence of two details (in whatever present contexts) serves to increase the complexity and vigor of his response. The coincidence of the popping motor-cycle sounds with the appearance of the officer served to produce a response of such violence as strongly to resemble that of the original experience. On the other hand the frequent encounter of sounds in new contexts served in time to eliminate the neurotic response.

Organization of Potencies

Each consequent comes in time to be the expression of the total past history of the system in which it occurs. The potency of a detail depends, among other things, on the nature of all other details, from whatever contexts, present in a given situation. Inas-

much as the tendency of each detail is to provoke consequents appropriate to its own past contexts, it is clear that the effect of a given stimulus pattern may be described as the resultant of numerous tendencies, each related to the past contexts of the stimulating details comprising the pattern. It thus becomes constantly more and more difficult to trace the causal relations of antecedent and consequent. For this reason psychology is more successful in its attempt to describe the earlier activities of any system of events with which it deals. Thus experiments with children are often more instructive than those with adults.

S was observed to be less liable to his uncontrollable activities when on the streets of a city near the hospital. Even loud and sudden noises then had less effect upon him, apparently because concurrent stimuli worked in other directions; the situation lacked a military background, or comprised many other backgrounds as well. Passers-by wore civilian clothes; no romping comrades bumped against him; sounds were accompanied by sights of wagons, workmen, trucks and like objects. But a dog-fight on the street suddenly threw him into a typical attack.

Variation in Antecedent Contexts

Some total antecedent contexts endow their details with greater instigative potency than do other contexts. Since a given detail may have participated in many contexts, its particular redintegrative influence on a given occurrence will also vary with differences in the past contexts in which it has appeared. Some contexts of stimuli seem to require repetition before their details acquire that potency which other details, in other antecedents, display after a single occurrence.

S was much improved by putting him to work in the Curative Work Shop, where he was encouraged to use noisy tools and to encounter loud sounds in new contexts, especially loud sounds over which he had control and for which he could listen in advance. These activities and new contexts served clearly to modify the redintegrative effects of noises and other sudden shocks but it was necessary to repeat these experiences many, many times before they appreciably modified the effects of the single experience at the battle front.

It was quite astonishing to observe the special potency given to sounds, sights and contacts by this single battle experience. Such events S had encountered throughout his life (a quarter of a century) but this particular situation was so impressive that more joyous contexts, such as Fourth of July celebrations, hunting expedi-

tions, and the like, were quite overweighed and obscured. In such a complex system as a soldier it would be impossible to trace the elaborate way in which all these ordinary contexts conspired with the impressive one, either by way of reinforcement or by way of inhibition.

Varieties of Incomplete Stimuli

Considering the stimulus or antecedent we may indicate *three conspicuous modes of incompleteness,* in all of which redintegrative potency may be present. The *part* that functions for an antecedent total context may be:

1—Some fragmentary detail, which in its present form, or in cooperation with other such details, was an effective feature of the original antecedent. In the case of S the sustained salvos of cannon fire required at the military hospital on the day of an Ex-President's death threw the patient into a pitiful state of relapse. Such explosive noises were an integral part of the situation leading to his original collapse.

2—As the result of the original experience a similar stimulus but one of lower intensity or of smaller magnitude may acquire an efficacy which it did not formerly possess. The partiality of the detail may thus take the form of a slighter degree or smaller amount of the original stimulus material. Thus the fear reactions of S were, in the beginning, made only to sounds that were terrific explosions. But in his later days at the hospital, noises which seemed to others quite ordinary were for him very effective stimuli.

Redintegrative activity is present even in those cases in which the only change in the stimulus is an intensity reduction. In this form the continuous gradations between redintegrative (mental) and non-redintegrative (physical) sequences is clearly seen. Simple muscular acts come to be executed with *less effort,* as the result of repetition and practise; even "purely mechanical" machines (automobiles, windmills, typewriters) require *less energy* for their operation after a preliminary period of use. The wrinkles of one's coat, once established, recur thereafter "on the slightest provocation". It is manifestly impossible to draw a sharp line between mental and physical, even in terms of the redintegrative sequence.

3—In the third variety, some coincident, accessory and originally irrelevant item tends to become "a part" of a context along with which it has several times occurred. The response to which it would have led independently becomes also part of the total consequent or co-response, although perhaps a very obscure and often overlooked part. The accessory item thereupon serves to provoke not only its own characteristic consequent; it instigates that co-response of which this minor consequent is now a constituent.

Thus the earlier reactions of the soldier to the appearance of a uniformed officer might have consisted merely or chiefly in attentive visual scrutiny of this individual. After his experience on the battle field, and throughout his hospital life, such spectacles as officers were constant features of the situations leading to his neurotic behavior. It was an

officer who managed the gun salutes; who supervised the weapons; who shouted loud commands. After many repetitions of this concurrence the spectacle of an officer led not only to visual exploration, as formerly, but also to the neurotic trembling.

Many observations of this pattern of redintegrative sequence have been made on lower animals, such as dogs. Thus the dog secretes saliva following the *taste* of food. But the *sight* of food leads first only to careful visual inspection and sniffling exploration. The *sound* made by a bell leads at first chiefly to a pricking up of ears and turning of the head. But if the dog is repeatedly *shown* the food each time he *tastes* it, and if on such occasion also a bell is *rung*, these co-stimuli become effective joint details of a complex antecedent which includes also the taste of the food. Thereafter the bell leads to the co-response,—pricking up ears, visual attention, salivary secretion. The sight or even the taste of food now leads not only to salivary secretian but also to concurrent activity of eyes and ears. All these may be somewhat modified in their integration, as indicated by the foregoing general laws.

Such cases as these last described have been named conditioned reflexes or conditioned reactions, under the misguided impression that only salivary secretion, in which the physiologist is especially interested, comprised the response, and that this response was in some obscure way "transferred" to the new stimuli.

It should be clear from the account we have given that we consider the so-called "conditioned" reaction but an instance of one of the modes of redintegrative sequence. It is what happens in a redintegrative situation when the "independent" responses to the accessory stimulus are obscure, or overlooked, or of little practical interest to the observer. The conditioned reaction, in its classical form, is but a half-described, misunderstood and often artificially produced instance of redintegrative sequence. Properly interpreted, however, it affords a convenient situation for experimental analysis and measurement. But for systematic purposes it is a miscarriage of observation.

Readiness of Response Features

Considering the elements of the consequent rather than those of the antecedent, it is to be noted that *some features of a co-response are more readily redintegrated than are other features.* The particular character of a response, and the various types and degrees of completeness which it exhibits, are thus also functions of the original response pattern itself. The influences responsible for such variations are very complex and must be independently indicated in various fields.

But we have already noted that, in the case of S, trembling was very easily produced; stammering followed only more sudden or violent sounds or shocks, whereas the visual pattern of an officer

would induce trembling but not, ordinarily, marked stammering. For the whole set of responses (trembling, stammering, fleeing) to appear, required a combination of circumstances, or very unusual cues, such as a dog fight. Some features of a response are thus more easily redintegrated than others.

As time went on and all kinds of stimuli were encountered in a variety of contexts, S gradually improved until, not long after the armistice, he was discharged as recovered. The occurrence of the armistice served to change the contextual character of almost every stimulus,—every experience was now a step on the way home instead of toward the trenches.

Synthesis of Response Items

A detail common to two former antecedent contexts, or two details from different contexts, may, as we have seen, lead toward two responses that may or may not be compatible, because of the degrees of congruity or incongruity of some of their constituents. In such a case *the redintegrated consequent often takes the form of some resultant or synthesis* of the two patterns. What form the resultant takes, as a function of response details alone, will depend on such factors as the mechanics and structures of their execution, as for example the muscles and levers (bones) involved, the nervous pathways concerned in innervation, and the various chemical, electrical, physiological processes correlated with the respective consequents.

As we have seen, S tended to react to the appearance of an officer by general incoordination and trembling. But by virtue of other contexts in his past, he also tended to react to such an object by movements of optical fixation and exploration. The actual eye movements on given occasions were observed to be a resultant of these two modes of activity,—tentative and unsteady visual regard, interrupted or modified by furtive glances of evasion; or a general pointing of the eyes in the direction of the officer, with the axes of vision parallel, as if looking beyond him at some distant object on the horizon.

Subjective and Objective Redintegration

We may define subjective events as those not open to general inspection, or those inconsistently reported. It is then of considerable systematic importance that *subjective as well as objective events participate in redintegrative sequences.* The events we have just been considering were the activities of the soldier,—his conduct as an organism in the space and time of the public world.

But S reports that other events, not accessible to our general inspection, also occurred. Not only did he see his own trembling and hear his own stammering voice. Sudden stimuli, he says, were followed also by feelings of confusion, dread, excitement. There were "prickling" sensations, wide-spread strains and tensions, "numbness" in parts of his body, and sometimes what he called "pictures of the battle" and "gloomy thoughts about the war."

None of these events were observable by those who watched him, although they are perhaps readily *conjectured* from his words, and *inferred* from much of his conduct. They were, however, events in that private region of nature often called "his consciousness." We judge them to be like experiences of our own, just as we conceive the trees he reports to be very like (if not entirely identical with) the trees we know.

Nor is the mere correlation of such events with that of his behavior which we can observe or with assumed events in his nervous system an adequate *description* of them. What S calls his *depression* may be correlated closely with what we call the tonus of his musculature; his *disagreeableness* may depend on interfering nerve impulses in his body; what he calls *tension* may be determined by the shortening of his muscles or by the simultaneous contraction of antagonistic muscle groups, and so on. S has nearly as good an opportunity to observe his nerves and muscles as we have, and in precisely the same way. But he would never confuse his gloom, his unpleasantness, his excitement or tension with these neuro-muscular structures.

Nerves and muscles, as known to physiology, are physical, in the sense this time that they are consistently reported patterns, chiefly visual in quality, with particular and elaborate spatial, temporal and mechanical relations. Dread, depression, excitement and tension are not visual patterns at all, however intimately they may be correlated with such events.

It has been suggested in the history of psychology that these are "two aspects" of some "one thing". But such a thing is not a natural event; it is a conjecture. And under any circumstance it is not the neuro-muscular system any more than it is the gloom or tension. They are different events, however closely associated, and only gratuitous mysticism can insist that they are two aspects of some one and unobservable thing. Yet both occur in nature, and with manifold correlations. Our present interest lies in the significant fact that redintegrative patterns are not limited to the muscular and glandular activities of organisms. Such variably reported

events as strains, moods, images, aches, pains, itches and thoughts also participate in redintegrative sequences. Subjective as well as objective events have their activities, and may exhibit behavior that is to be considered mental. In so far as this is the case, both exist "in the mind."

The Nature of Mind

Mind is but a name we give to natural sequences of this redintegrative type. The past context, once passed, has become a fact of history. Its efficacy in the present is not due to its "reproduction", nor to "copies" of it that are in any way preserved. To be sure, reports of it may be given, as in speech and in writing. Inanimate evidence may be found which leads to the continued inference of its having once occurred. And there is some evidence that quite new events, called "mental images", may occur which present many of the relations of former episodes. But the past context does not become effective through any magical mode of revival or perpetuation. Instead it is the case that some present detail acts as a surrogate for the past context, resembles it in instigative efficacy. In this sense the present event may be said to represent, stand for, mean or point to the past or absent.

These redintegrative activities of the soldier are what we mean by his mind. It was because "his mind was affected" that he was sent to a hospital for functional mental conditions. S displayed mind or mental activity, that is, he reacted to signs or details *as if* wholes had been present. Nor did he first hear the sounds (for example) then revive "in his psyche" a reproduction of the battle scene, thereupon reacting to this ghost of an event long gone by. There was no "reinstatement" of the past in his world of nature. Instead, some present detail always functioned for past contexts. The mind of the soldier is this repertoire of responses to symbols of past or absent contexts.

Nor did the past contexts exist "buried in the unconscious". Unless the unconscious means only the past the words are nonsense; and if they mean the past, the words are superfluous. The past events have their place, to be sure, but their place is in history, not in realms of unconscious ghosts of bygone events.

Summary of General Redintegrative Laws

1—Identical events are those persisting or belonging in a class.

2—Consequents may follow partial details of their former antecedents.

3—The completeness of a redintegrated response varies with the completeness of its stimulus.

4—Some of the details of an antecedent context have special instigative potency.

5—The potency of a given detail varies with many determinants.

6—A detail's momentary potency is a complex resultant of all its effective determinants.

7—Stimulating details may reinforce or inhibit one another.

8—Each consequent comes in time to reflect the total past history of the system in which it occurs.

9—Some antecedent contexts endow their details with superior instigative potency.

10—Varieties of stimulus incompleteness may be classified as those due to (a) partiality; (b) reduction; (c) concurrence.

11—Some features of a co-response are more easily redintegrated than others.

12—Simultaneous response tendencies combine or synthesize in various ways, producing resultant patterns of compatible features.

13—Both subjective and objective events participate in redintegrative sequences.

14—The efficacy of a past context is not due to its reproduction nor its unconscious persistence; some present detail functions as its surrogate.

15—A mind is the sum total of redintegrative activities exhibited in or by a given system.

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PERSISTENT PROBLEMS IN SYSTEMATIC PSYCHOLOGY
IV. STRUCTURAL VERSUS FUNCTIONAL ANALYSIS

Raymond Holder Wheeler

In the preceding three papers we have sharply distinguished between two viewpoints, the structural and functional, the logical assumptions of which must apparently be kept clearly in mind if we are to handle scientific problems unambiguously and avoid pitfalls in interpreting the systematic relationships of scientific data. In the first of these papers we discussed the manner in which the structure-function problem as it developed in philosophy was distorted with the result that psychology inherited a dualistic conception of mind and body. In the second we traced difficulties with the psychological datum due to confused applications of the structure-function logic. In the third we attempted to point out certain additional difficulties which could be traced to the same source.[1]

In this paper it will be our purpose to show that the recent emphasis in psychology upon functional analysis with its skepticism toward the structural approach to psychological problems is still another violation of the structure-function logic. The two viewpoints lead to mutually exclusive accounts of the datum. It is obvious, therefore, that the two methods of analysis lead to facts which are used for totally different purposes. Yet the methods are mutually dependent, and results from the one are adequately understood only in the light of results from the other. Ideally the two should be undertaken simultaneously in the pursuance of experimental problems.

Structural analysis leads to the "whats" of experience, or the "whats" of nature. Functional analysis ascertains the "whens" and "whys".[2] By "what" we do not imply a metaphysical problem such as is inferred by the question, "What is red?", nor do we imply the "what" inferred by such questions as, "What is perception?" and "What is configuration?". Such questions depend for their

[1]Persistent Problems in Systematic Psychology. I. A Philosophical Heritage, *Psychol. Rev.*, **32**, 1925, 179-191. II. The Psychological Datum, *Psychol. Rev.*, **32**, 1925, 251-265. III. The Stimulus Error and Complete Introspection, *Psychol. Rev.*, **32**, 1925, 443-456. See also the *Monist*, **33**, 1923, 556-567, for the writer's discussion of the general problem of the static and dynamic in the logic of science.

[2]We shall state later our reasons for thinking that "functional analysis" is misnamed.

answer upon generalizations from experience or observation, and lead to definitions. We simply mean content, and seek an answer to the question, "What is present?", or "What is going on?", or "What is the individual feeling, seeing, hearing, doing, *et cetera,* under the conditions of experimentation or observation?". Such "whats" as these are merely predications upon experience—simple statements of fact. The key-word is description.

This is all that introspection is—all that it ever was, intentionally. It was a legitimate desire to keep this description under control that led Titchener and his followers to distinguish between process and meaning. The fact that such a distinction failed of its purpose has nothing to do with the adequacy or inadequacy of structural analysis as such. It undoubtedly suffered through a neglect of functional principles in general. Functional analysis is without question one of the most important factors in securing adequate description. On the other hand structural analysis, we hope to prove, is *equally* as important in determining adequate functional analysis and in defining the results obtained therefrom.

Recent attacks by the configurationists upon the work of the process psychologists and introspectionists[3] have been so vigorous and detailed that a somewhat thoroughgoing discussion of the whole matter seems worth while. We sympathize heartily with the position that structural analysis, or description, alone, will not yield information which can be used in prediction and control directly. On the other hand the tendency thoroughly to condemn almost *in toto* the work of one group because that work contains certain faults is altogether unfortunate, at least until the logical basis of that work, and therefore its right to serious consideration, has been very carefully inspected in all of its aspects. The method of the process and introspection psychologists has been attacked on the basis of its results without sufficient regard for the logical justification of the method. We shall attempt to show that the method is not only logically justified but logically essential and that the unsatisfactory status of results at present is not due to the method *per se* but to the conditions under which it has been used.

As we have defined it, structural analysis is an end in itself with reference to the interests and ambitions of a particular scientist,

[3]See especially M. Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.,* 1, 1922, 47-58; 4, 1923, 301-350; K. Koffka, Zur Theorie der Erlebniswahrnehmung, *Ann. d. Philos.,* 3, 1922, 375-399; Koffka, Introspection and the Method of Psychology, *Brit. J. of Psychol.,* 15, 1924, 149ff.

not with reference to the character and value of science.[4] The "structuralist" makes the mistake of considering analysis systematically as a goal. The classical view that science is pure description leads to the implication of a static universe. In psychology the configurationists are seemingly right in their insistence that this viewpoint is sterile of results which as such lead to prediction and control, and that the scientists' guiding principles are (1) an interpretation of the universe as dynamic, and (2) prediction and control.

The rôle of structural analysis is therefore that of a means to an end. We commence in any scientific research with a common sense knowledge of the content of nature. Certain of these contents or forms are later abstracted from their setting for practical experimental reasons. But how do we know with what to begin? How shall we begin? Where shall we begin? Certain events (the functional corollaries of content, structure, form) stand out

[4]There seems to be a tendency at present to permit mathematics and the logic upon which it is based to determine ultimate interpretations in all science. We believe that this is a biased conception. Accordingly the arguments of this paper rest upon the following assumptions. Whether they in turn show a bias, only the reader can judge. (1) In science we investigate a world of content-in-action, and from the standpoint of psychology this includes the abstracted content, mental process. (2) Content of any kind is discovered empirically by direct observation, sometimes with the aid of instruments of precision and with various methods of control. (3) Direct observation is logically primary but in practice must be supplemented by hypotheses and as rigid a control of observational conditions as possible. In turn observations thus made lead to such changes in the controls and hypotheses as the experimenter desires and the logic of the situation demands. (4) In ascertaining the laws of content-in-action we employ concepts certain of which may be expressed symbolically in terms of mathematics. These concepts serve the two purposes of picturing or reflecting contents in the concrete, in their relationships, and of stating uniformities. Throughout it is the purpose of science not only to reduce knowledge to abstractions and laws but to furnish us with a clearcut and detailed picture of concrete things. Abstractions are meaningless out of relationship to the concrete. Conversely facts about the concrete have no scientific value except as they possess general validity.

If we shape our concepts of the universe exclusively in terms of mathematics and assume that any ultimately worthwhile description is wholly mathematical we neglect what seems to be an unavoidable principle that mathematics ultimately points to a world of actual content—actual phenomena. It directs our description of these phenomena but does not furnish us with that description. Insight into the laws of nature has increased by leaps and bounds with the aid of mathematics, and through its influence, perhaps, we have dispensed with that troublesome thing called quality. But this does not do away with content and form, i. e., structure.

in direct observation and interest us. We know enough about the controlling conditions to believe that refined procedures will be worth while. Now up to this time the data have been derived from structural analysis—analysis of total situations in which the relationship between one event and another have been comprehended superficially. A total situation, undifferentiated, vague and indefinite as to details, has already broken down for us into its component parts. In the laboratory we do nothing more than to refine the crude and common sense procedure already commenced.[5]

If structural analysis furnishes us with the information with which a science begins may we ask just where in the development of that science do we discard it? On the contrary take a problem into the laboratory and structural analysis still plays the same rôle of means to an end. The experimenter faces the task, always, of relating his facts to the rest of experience; he must ascertain whether the process he is endeavoring to control is actually taking place or not; he must name it, classify it, compare it with processes observed by other individuals under like conditions; watch the process change when conditions are changed. He must identify the process, although the identification may not be absolutely exact. None of these tasks are fulfilled in the absence of direct observation, which always involves structural analysis. Functional analysis, which yields the conditions of an event is employed in defining the event in question but only by direct observation can it be *recognized*. Indeed, without structural analysis how would we conceive of functional problems and carry on experimental procedures? How could there be any knowledge whatever? How does one predict, measure and control without something to predict, to measure and to control? Call that which is controlled whatever one likes, and assume it to be as arbitrary or as illusory as one's conscience will permit, it is still one aspect of the world in which we live, one aspect of experience. We continue to give it a name in each special instance and our hypotheses as to what it is like continue in part to direct our investigations no matter how functional in viewpoint we become. "It" in the view here set forth may be stated in general terms as the phenomenal appearance

[5]For example, prior to experimentation in psychology, perception, feeling and thought were analysed from undifferentiated experience. Then we went further. Certain structural forms were distingushed still called sensory processes and images. Notwithstanding ambiguities of definition these remain characteristics of structure better names for which have not been found.

of whatever is under observation—in commonsense language a "thing," a substance, a compound, a rock, a sense impression, a thought. In more refined language it may be called a pattern of electrical charges, a center of force, a configuration or what not.

If any other scientist than a psychologist wanted to make careful measurements of a certain process he would not only guarantee as great a uniformity as possible in his data by judiciously controlling his experiment and selecting his material but he would pay particularly close attention to the evidence of his senses regarding the object under investigation; and were his senses keen enough to make more detailed studies with or without the aid of instruments he would most certainly do so. This evidence obtained through the senses corresponds to introspective analysis in psychology. Any tendency to belittle the importance of such analysis betrays the purpose for which psychological work is done. It shows a prejudice toward functional analysis and away from content as great and as dangerous as any introspectionist has shown toward content and away from function. We shall accomplish little in the long run by taking either extreme position.[6]

In physics and chemistry the shift of viewpoint from the structural to the functional does not lead to our hearing less about the structure of gases, solids, metals, compounds and their ingredients —the structural forms of the physical universe. On the contrary functional analysis in physics and psychology alike makes the problem of structural analysis all the more acute and challenging. The finer we make the one the more at a loss we are until we also refine the other.

True that the distinction between structure and function is in all science becoming less explicit (except in psychology) because the

[6]One sometimes hears made the remark that the psychologist, like the physicist, should bother himself less with indeterminate qualitative aspects of his phenomena. Leave that to the philosopher! For example the physicist does not know what electricity is and yet functional analysis has led to remarkable measurement and control. Exactly, but the physicist has a wealth of information and hypothesis about electricity falling on the structural side of our dichotomy—electrons, protons and systems thereof, flocks and migrations of ions, liquid, colloid and solid "states", wave form *et cetera.* Indeed all the facts of change are stated at one time or another in structural language. (See the writer, *Monist,* 1923, Vol. 33, 556-567.) Here again let us keep in mind that while these facts are *determined* through functional methods, they are *discovered* through structural analysis.

Of course phenomenological descriptions in physics consist to a considerable extent of imaginative representations or symbols, in lieu of direct observational data; but they are not imaginative by choice.

issue is becoming better understood.[7] It is not becoming less important. For example the long-lived distinction between anatomy and physiology is disappearing. The anatomist is more and more resorting to functional analysis, which simply means that his interests are in ascertaining changes in structure when conditions change. He is studying structure not as an end in itself but as something which functions. But his structural and anatomical facts become none the less important because he uses them as he goes along instead of filing them away on library shelves. We may cease eventually to have separate departments of anatomy, physiology and zoology in our universities but this will not change the systematic relationship between structural and functional analysis or change the logic of science. The structures of organisms as well as the functions will still be the objects of investigation. The present trend in biology is a division of purpose and interest not a disregard for the facts of anatomy or a rejection of structural analysis.

As we proceed from the physical through the biological to the psychological sciences the importance of direct observation and analysis becomes greater and greater. There are various reasons for this: (1) the psychological sciences are younger and the repertoire of structural information which guides us systematically through functional problems is therefore less adequate. (2) Our ability to control experience with extra-organic aids is less because of the variability and complexity of the processes under investigation. This makes it all the more necessary for the observer to describe in detail what he observes, in order that the experimenter may know when he gets comparable results. The organism as a whole presents difficulties of control chief among which is the contribution to the event investigated which the organism itself makes. This difficulty is controlled partly by a knowledge of the history of the event and by the organism's description of that event. There is a possibility that the individual can carry out a certain activity in more than one way, even with conditions kept as con-

[7]Our acquaintance with structure in physical and biological science has grown to such an extent that systematically the facts are taken for granted. Content is referred to less frequently in its relationship to function, for familiarity with the former seems to have bred for it a contempt which is fast developing into a logical skepticism. The instant structure is systematically slighted there is nothing to prevent a denial of content altogether, nothing to hinder science from becoming a set of meaningless mathematical abstractions. Already the physical scientists are beginning to substitute God for content!

stant as possible. This adds to the desirability of introspective description. In a simple psycho-physical experiment, for example that of lifted weights, one individual may use visual imagery as a cue while another may employ kinaesthesis. Examples of this sort are innumerable in the various types of experimental work in psychology. (3) To ascertain lack of uniformity in the data is important for an adequate understanding of human nature. Structural analysis is the procedure by means of which these individual differences become known.

The argument so frequently used against introspection, namely that it is impossible to standardize it, is hardly worth taking seriously until we forsake arm-chair criticisms and set to work demonstrating whether prejudiced observations can be avoided. Before this type of structural analysis can be relied upon, the subject's protocols in a specific investigation from the time he was a naive observer with little or no knowledge of psychology to the time he is said to be a trained observer should be published, together with the questions asked him by the experimenter and a statement of the subject's beliefs or opinions relative to the nature of the experience he attempted to describe. Much valuable information which would unquestionably point to an adequate standardization of introspective technique has been thrown into wastepaper baskets! With a genetic picture of the observer's reports available the reader would have some basis for judging whether the reports show a prejudice, especially if the reader had taken the pains to secure a genetic picture of his own progress in making reports of like nature. Much that seems to be a lack of uniformity in report is only a difference in the choice of words. On the other hand much of the criticism which has been leveled at introspection has come from those who have not taken the pains to carry on any serious work of like character. Their own prejudices against the method have motivated their attacks, rather than their faith in results which they themselves have taken the trouble to accumulate. Until the spirit of meeting results with results dominates in this field, we shall continue to have avoidable controversies.

II

Turning now to some of the specific charges made against analysis by the configurationists and to their definition of functional analysis, we find, first of all, a logical difficulty in their attempt to subsume the descriptive and functional aspects of exper-

ience under the same concept.[8] Experience as known is all structure, all content. Its functional aspect is always an inference, drawn from watching changes in structure under controlled conditions. The dynamic aspects of experience are *lived*. They are *observed* as *content*. Consequently that concept under which the configurationists attempt to subsume both the structural and functional aspects of experience is unattainable. It is a *Ding an sich*, a phantom, a logical construct without a name. We can agree with them that metaphysically and epistemologically we assume a reality of which structure and function are two aspects, but in practice there is nothing to do but to consider them separately, the one presenting a problem of description, the other of interpretation. The errors, ambiguities and artificialities which follow from this attempt to put the structural and functional aspects of experience into the same category are numerous and far reaching. We have time to consider only a few of them.

First is the implication that by limiting the stimulus-situation structural analysis takes care of itself, or that it is accomplished by the experimental setting. Structural analysis is accomplished only by the observer himself, by the knower. The determination of scientific facts by functional analysis is not a substitute for the discovery of those facts. Causation is not a synonym for analysis. If there is any reality at all external to experience, "out there" structure and function are one. There is the "thing" which the configurationists would like to subsume under one category. To be sure we are now discussing epistemology and a theory of reality. But the structure-function problem carries us that far and since the configurationists have sharply defined the issue after accepting the dichotomy by ruling out in practice one half of it, we must meet implications with assumptions.

A structure-function system is a parallelistic unit, a logical not a metaphysical or epistemological dualism assuming two qualitative ultimates. This parallelism, however, is so exact and rigid that the intelligibility of scientific results hinges upon a simultaneous refinement both of structural and functional concepts. At the same time that the physicist thinks of electrons as energy, he speaks of them as contents. He refines his structural terminology as best he can in the absence of concrete observations by the use of such terms as "particles of matter", "quanta" and the like. His phenomenological accounts become more and more speculative as his data become more and more infra-observational, but in compre-

[8] *Ann. d. Philos.*, 3, 1922, 375-399, (K. Koffka).

hending the relationships between one "form of matter" and another his imagination cannot be any the less exact in its logical aspects. Though the psychologist may think of a "mental process" as an act, adjustment, or response, that activity is unintelligible until described. It may be intelligible to himself because he observes it without the necessity of communicating his observation to others. How else than by description can he make the observation intelligible to others?[9]

A second error is that of proceeding to the opposite extreme from the process psychologist in postulating the goal of science. To control and predict is apparently the sole aim. But quite as fundamental is our effort to *understand* the universe. Indeed without such an understanding whether right or wrong, prediction is a simple matter based on statistics. Any one can predict that the sun will rise tomorrow morning or assume such a prediction by his behavior without any comprehension whatever of the relationship between the sun and the earth. Likewise control is easily established purely on the basis of statistical data with no understanding of what nature is about.

If we follow out the logic of the purely functionalistic position, understanding the universe amounts to a resolution of it into a barren mathematical formula, excellent because of the exactness of the truth which it represents, satisfying because of its simplicity, pretty because of its convenience. All to the good! But beautifully devoid of reference! We have the prediction and control which we greatly desire but nothing to work it on! After all, functional analysis is as much a means to an end as the structural. The former is a means to prediction and control and a stepping stone in the refinement of structural analysis, while the latter is a means to functional analysis. When it comes to understanding relationships, functional analysis gives us our cue as to what the relationship is; structural analysis and structural hypotheses yield the terms which are related. Accordingly an understanding of the universe hinges as much upon the latter as the former.

Thirdly is the configurationists' discovery that the behaviorists and process psychologists do not account for behavior and the facts of mental life. We are told it is because structural analysis takes activities out of their setting and therefore yields meaningless abstractions. The impotence of behaviorists and process psychologists in this respect is not due to structural analysis as such, but

[9]Cf. the writer's "Introspection and behavior", *Psychol. Rev.*, 30, 1923, 103, for an attempt to show how introspection and "objective" observation do not differ in this respect.

to their neglect of functional analysis and to their illogical attempt to account for the properties of wholes in terms of the properties of parts. It is only an historic accident that atomism and structural analysis have gone hand in hand.[10] While breaking up of wholes into their parts is an essential aspect of certain scientific procedures, it is not essential that *ipso facto* these wholes be accounted for in terms of their parts, at least exclusively.

Fourthly, structural analysis does not necessarily lead to elements or fixed quantities. It yields "convenient entities." It does not lead necessarily to an accounting of the complex in terms of the simple, but it is essential in understanding the evolution and differentiation of structure, also in ascertaining whether we are investigating something relatively simple or relatively complex and what constitutes their differences.

Fifthly, the configurationists show their bias in insisting upon naive observers in an effort, presumably, to avoid structural analysis. The real—possible not rationalized—purpose is evidently that of avoiding prejudiced observations. If superficial, undifferentiated and uncritical observations are all that the psychologist is interested in, such a position would be comprehensible. Naive observers are in a predicament analogous to prejudiced ones in not being able to tell the difference between what they see and what they think they see. They are less capable of seeing anything since they are lacking in powers of discrimination. They are like the would-be naturalist who can neither navigate or make discoveries the first time he enters the forest. The situation to which he is reacting has little if any meaning.

Sixthly is the charge that sensations and behavior-units are the the products of reflection, with the implication that configurations are not. We have already dealt with this situation in our discussion of the psychological datum.[11] What we there said with reference to process and meaning holds, we believe, for part-whole relations—for analyzed and synthesized experience. Any experience contains an interpretive factor. No experience is immediately given or wholly derived. Immediate experience and reflection are genetic stages in the emergence of one pattern reaction, roughly the beginning and the end. If a configuration as a whole is given in experience it is equally a product of reflection. It would be the same for sensation as for a *Gestalt*. Moreover a configuration is

[10]Just as materialistic and mechanistic theories have been associated by accident and not by logical necessity. Atomism is merely a bad application of the facts of structural analysis.

[11]Persistent Problems, etc., *Psychol. Rev.*, 32, 1925, 251.

as much a unit in its relations to other configurations as a sensation or an image in relation to other mental processes. The relationship between the unit and the whole or anything more complex than itself is a problem independent of the epistemological question of what is given and what is reflection. Here again the configurationists seem to have confused two issues, the relationship of part to whole and the epistemological problem which pertains equally to the part and the whole.

Seventhly, our critics attack structural analysis in psychology as if it were exclusively a matter of arriving at sensations or mental elements on the one hand and chain reflexes on the other. Obviously by structural analysis we arrive at something but how we shall systematically regard that something is the product of an entirely different procedure based on other assumptions. *If* we assume that we can explain the complex in terms of the simple we employ the products of analysis to that end. But *if* we assume that we cannot so explain the whole in terms of its parts the products of analysis are used in finding out what changes take place when its parts are affected, in ascertaining the conditions which may bring about a synthesis, in refining the conditions of functional analysis and in many other ways too numerous to mention.

Eighthly comes the assertion that we cannot get together again that which we take apart with the implication that therefore the products of analysis are meaningless artifacts. Why this obligation? Nature takes care of synthesis! Even should we be able to bring about a synthesis experimentally we have done no more than to give nature the chance. *We* do not put the "parts" together. If an observer analyzes a feeling or thought we would hardly expect him to synthesize the remains by an act of will! On the other hand it is a simple matter to set up conditions which would produce another feeling or another thought similar to that previously analyzed. It is not of course necessary to insist that before the products of analysis can be regarded as thoroughly checked the pieces must have been put together again. Inability to find a remainder, following the analysis, is a check which anyone may carry out if he chooses.

Ninthly, we read the criticism that knowledge of the parts will not enable us to predict to the properties of the whole. Perhaps this statement is correct in its entirety and perhaps not. We are apparently justified in assuming that by "whole" we imply parts and that the whole does not exist without its parts, which is to say that parts are essential conditions of wholes. But who wants to predict to the properties of the whole as such, or at least all of

them, from a knowledge of the parts? That the structuralist ever made such a claim for himself or his method can be seriously doubted. To criticize him for not being able to do it seems hardly to the point and assumes an objective in which the introspectionist is not, *qua* introspectionist, interested. The writer, for example, once conducted some research on choosing.[12] It became evident that the only content which was invariably present was kinaesthesis. When the subjects were asked to choose their alternatives without making incipient motor adjustments they found the task impossible. While from the standpoint of systematic psychology this seemed to be an interesting and perhaps valuable fact, one would certainly not attempt to predict on the basis of such information why an observer would choose one alternative rather than another or what motive might lead to any particular decision. Nor would one define choosing in terms of kinaesthesis. Whether the knowledge that all choosing involves kinaesthesis will ever be a fact employed directly or indirectly by the functionalist, or one pursuing a functional problem, remains to be seen.

Meanwhile, tenthly, we hear that the products of structural analysis are not the real data of experience. To this we reply, in addition to what we have said previously, that what constitutes any particular class of wholes is variable and the components of any particular whole are somewhat indeterminate. Under similar experimental conditions for one observer a whole is one thing and for another observer another thing. Whatever experience is reported by a single observer is no doubt a whole, a configuration, but it may correspond to a whole-part of the whole which another observer is reporting. Analysis changes one whole to another.

Inspect the configurationist's view as it applies to mental growth and assume that the child's first perception of the mother's face takes in the face as a whole. It is a "friendly face". There comes a time when the child discovers the mother's nose. The original configuration has changed. It will never be the same face again. Should one say that this analysis is not real?

Strictly speaking, we study no broken down experiences anyway, only wholes. The instant an experience is broken down it is not that experience any more. It is something else, certain aspects of which seem to be essential to the existence of the original experience. Let a student enter an experimenting room and approach the apparatus. As he prepares to make observations his experiences are being narrowed down, controlled. Perceptions of total situations are breaking down under a selective process. This is struc-

[12]University of Oregon Publications, Volume 1, No. 2, 1920.

tural analysis insofar as it is controlled by his attitudes and mental sets and insofar as he is led by the experimental setting and instructions to anticipate certain experiences rather than others. Is all this unnatural and are the results to be unreal? If so, the configurations are studying unreal processes and similarly is every scientist. Again, when the observer sees through the microscope after practice things which he was unable to see before are his observations unreal? Apparently in terms of the configurationists' definition of unreal and unnatural experiences no products of selective agencies or results of practice could be real and natural. Any disintegration or destructive process in nature would yield consequences which are not real!

Another point: Phenomenologically the experience under investigation in the psychological laboratory may be one thing when apprehended as a whole but quite a different thing when functioning as a member of a larger configuration. Would the configurationists call it real in the one case and not in the other?

The experimental situation, any life situation, compels the observer to analyse. If in the laboratory he resists analysis, allows his experience to lose its identity and detail by merging into a larger configuration he would be charged with stupidity! Of course the external experimental situation is by no means the exclusive determiner of what the subject observes. His attitude, previous experience and the like, even under conditions of functional analysis, are of paramount importance.[13]

One might just as well insist that synthesis yields an unnatural product because it changes the status of experiences which otherwise would be analyzed or separated from possible wholes. Water is unnatural because it contains hydrogen and oxygen; a thought process is unnatural because it involves imagery; a configuration is unnatural because it contains figure and ground! The argument works both ways because the products of analysis and synthesis are relative. What is the product of analysis in one situation is synthesis in another depending upon the point of reference used—the levels of complexity which one is for the moment considering. For example, picking out the word "man" from its context is analysis. Apprehending the phrase as a whole is synthesis with respect to the word "man", and other words in the phrase. The fact that one can concentrate on the word "man", but only by sacrificing for the time being the meaning of the phrase as a whole, in no way makes details about the perception of "man"

[13]Cf. Harry Helson, The Psychology of Gestalt, *Amer. J. Psychol.*, 36, 1925, 494; 37, 1926, 25, 189, Especially 37, 204ff.

unnatural. Whether these details are relevant to a particular theory or purpose which the configurationists may have in mind is entirely a different matter. But these facts *are* relevant to psychology!

The configurationists seem to imply that analysis and synthesis go on only at different times under different sets of conditions—that the two attitudes are mutually exclusive. On the contrary both are constantly and simultaneously going on in the individual. Only as one changes his point of reference from something he has for the moment considered complex to something which he has temporarily considered simple, are the two procedures mutually exclusive. One does not analyze experience without at the same time synthesizing the products, not into the original whole but into different ones. Likewise he does not synthesize without at the same time analyzing the product out of a still larger whole. If there is any distortion it is going on all the time!

There is no experience which is not complex. Its complexity means that details are observable along with the whole. Within rather wide limits these can be observed without changing the total configuration. Take again the word "man". One may see it is a whole and grasp a suggestion at least of each letter without sacrificing the wholeness of the perception. If in general it were not for such a fact as this, how would we ever know the difference between something simple and something complex. The concept, complexity, itself, hinges upon our ability to apprehend parts and wholes at the same time or at least within a unified pattern. More than that, the concept "whole" presupposes structural analysis, the method by means of which we ascertain the existence of wholes.

The attempt to reject structural analysis is strikingly comparable to Watson's rejection of "consciousness". The latter attempt was based on a metaphysical dualism with two opposed methods of observation assumed, the subjective and objective. While the configurationists were too wise to follow a dualistic philosophy as Watson did, their desire for more adequate methods has led them to an equally as absurd and extreme logical position. They have made of the structure-function problem a methodological dualism as marked and rigid as the old metaphysical dualism of mind and matter. This methodological dualism to which they adhere is assumed in the rejection of one side of the dichotomy.

Many of the points which we have covered in this paper have already been brought out by Helson, however with less intent to

defend the principle of structural analysis.[14] Certain other weaknesses of the *Gestalt* psychology, from a logical standpoint, are there pointed out, all of which can be traced to a prejudice on the part of the configurationists against introspection. The vagueness of the notion, *Gestalt,* is due to a neglect of descriptive terms. There seems to be no reason why many of the facts from *Gestalt* and process psychology do not belong together. Those details which are said to have membership character in configurations are the details we have all along been calling sensory processes and imagery. By recasting definitions they might easily fit into *Gestalt* psychology. In a subsequent paper we shall endeavor to show how configuration psychology still needs the concepts of attention and association and is implying them right along. The important contributions (and there have been many) of the configurationists have been along methodological and factual lines, but systematic only to the extent of improving upon atomism. Emphasizing the whole in its own right gives us much that is desired. But this, incidentally, is not new.

There is seldom if ever a movement in psychology which is not extreme, and does not overemphasize a certain idea. Several years ago there was an interesting controversy between sensationalists and imageless thought advocates. Both the former who contended that thinking was phenomenologically reduceable to sensory processes and imagery, and the latter who held that certain thinking at least was imageless and sensationless were probably right. A thought process observed in its totality, as is done by the layman, for example, is a unitary process, sensationless and imageless. Without an analytical attitude none of these component processes are apprehended. The experience is "pure meaning". It is a thought whose properties are describable in terms of meaning.

That same thought, however, apprehended under the influence of an analytical attitude, loses its unitary and synthetic character to a degree comparable to the effectiveness of the analytical attitude. If the attitude is only partially effective the thought may turn out phenomenologically to consist partly of sensory and imaginal processes with an unanalyzed residue which is generally apprehended as pure meaning since its content is not perceived. Let the analytical attitude be complete and the thought changes to sensory and imaginal processes exclusively.[15] Such is the inevitable result of any complete structural analysis. The procedure is de-

[14]Loc. cit., 204ff.

[15]Cf. Wheeler, Analyzed versus Unanalyzed Experience, *Psychol. Rev.,* 29, 1922, 425-446.

structive. The results of the two schools are not incompatible at all!

While the following analogy should not be taken too seriously, it may prove suggestive. There may be very good grounds for saying that water as a chemical compound is hydrogenless and oxygenless in spite of the fact that these two "elements" are its ingredients. Both have lost their identity in the synthesis. Exactly in what phenomenological form they exist in water, or whether they really exist in water, nobody knows. We do know that water can be broken down into these elements. For the solution of many problems in chemistry it is of no particular value to know this fact. On the other hand for the solution of other problems this fact is essential.

Psychology seems to possess a corresponding problem. For the solution of certain questions it is of no particular value to know that a given thought process contains visual imagery, verbal imagery or what not. But there are numerous questions the solution of which is impossible without such structural data. It may be that a thought process in its synthetic form contains sensory processes and imagery in somewhat the same way that water contains hydrogen and oxygen. In the absence of structural analysis one would never be the wiser with reference to the "components". Without breaking down thinking, however, into its components the psychologist would never know how to relate it to the rest of experience.

We said earlier in the paper that structural analysis gives us the "whats" of experience, and that functional analysis informed us of the "whys". There is a sense in which structural data gives us information about the "hows". Function implies content. The content can be envisaged as the "how" of function. A man walks with his legs. His legs are structures with reference to the body as a whole. They are the "things" which move. Kinaesthesis, visual imagery, auditory imagery and the like are the "things" with which we choose and think, as described phenomenologically. They comprise the *modus operandi.* Chemical substances—structures—are the *modi operandi* of chemical interactions. Structures, as aspects of functions, are ways-in-which activities take place. They share equally as "hows" along with other sets of conditions of function. Cause is also interpretable as "how". In psychology, then, introspective data share equally with the stimulus situation in *accounting for* function. Thus we differentiate two sets of conditions of function: the excitatory, i. e., the conditions which set off the function (the situation of which the activity in question is a mathematical function) and the mechanisms of the function *as a*

process. The excitatory group of conditions inform us when, where and why the function occurs. The structural group informs us what occurred, that it occurred and tells us of the instruments with which or *in terms of which* the function *carried* on. The latter group can be ascertained only by structural analysis. Wherever it is impossible to make the analysis by direct observation we compensate by imagining what the products of direct analysis would be were our powers of observation fine enough.

To recapitulate briefly, we have attempted first to point out the scientific value of structural analysis and to show its relation to functional analysis. Secondly, we have tried to show that certain criticisms of structural analysis offered by the configurationists, who stress functional analysis, are based upon a confusion between the logical rôle that structural analysis plays in science and certain other assumptions which are irrelevant to the issue. Thirdly, we have contended that the emphasis of functional analysis to the neglect of structural analysis shows a prejudice as great and as dangerous as the prejudice shown by the psychologists whose work the configurationists have attacked.

We wonder, in conclusion, whether functional analysis should be called analysis at all since it is something which goes on external to experience, and is a method we employ to determine and control events, including experience. Is there analysis in a world devoid of experience? At least it is evident that we should sharply distinguish *determination* from discovery. Functional analysis, so-called, has to do with the former; structural analysis with the latter. We wonder if, after all, discovery is not the only kind of analysis to be found. At any rate it is structural analysis whether it is a discovery of a whole, its parts, or both.

We heartily and sympathetically grant that "functional analysis" is half of science. We insist, however, that with progress along this line there goes a greater need than ever for adequate structural analysis. The one is not a substitute for the other any more than cause is a substitute for effect. The introspectionist's work is raised, by functional analysis, to levels of greater importance rather than less. Let us not keep making the same systematic errors over and over again in psychology through a neglect to follow out to its end the logic of the structure-function antithesis.

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THE DEPENDENCE OF HUE ON DIFFERENCE IN TINT[1]

RUBY G. BROWN AND L. B. HOISINGTON

The problem of this Study grew out of some preliminary results obtained in determining the spatial limen for film colors.[2] The authors of the earlier Study state that their *O*s frequently reported the proper hue of the film color, at distances well beyond the average range of the experience of hue, when there was a difference in tint between the color and the grey of the screen through which it appeared, and that the reports of hue ceased when the color and grey were equal in tint.[3] We had, then, as our problem, to make a study of this change in the report of hue according as the tint of the color and of the grey of the screen was equal or unequal.

The apparatus[4] was, in general, the same as that of the previous Study except that, instead of adjusting the lights for equality of tint at all times, we introduced a sliding resistance into the circuit of one light in such a way that we could vary the tint of the one experience from very much lighter to very much darker than that of the other. We set our rheostat in the circuit of the light that illuminated the colored disc, and allowed the light that illuminated the screen and slide to remain constant, in order to keep the state of adaptation as uniform as possible. Since the tint of the disc must be lighter as well as darker than that of the grey screen, we placed the variable light so close that the tints were equal when approximately one-half the resistance was in the circuit. With this light 85 cm. from the disc, the color and screen were of the same tint when 8 cm. of a 5 ampere 40 ohm resistance coil, 22.5 cm. long, was in the circuit. This arrangement allowed 2 steps of 4 cm. each on the one side of equality; and we took 2 steps of the same magnitude on the other side. The five amounts of resistance gave five tints, the brightnesses of which as measured by the Holoplane Lightmeter were, from weak to strong, 1.06, 1.20, 1.43, 1.82, and 2.37 foot-candles.

These conditions gave a film color behind a screen of somewhat indefinite surface. For the color stimuli we used compound discs

[1]From the Psychological Laboratory of Cornell University: transmitted to the Editors by E. B. Titchener, H. P. Weld and L. B. Hoisington.

[2]M. Elliott, J. West and L. B. Hoisington, The Spatial Limen for the Four Principal Film Colors, *Amer. J. Psychol.*, **35**, 1924, 125 ff.

[3]*Ibid.*, 127.

[4]*Ibid.*, 127 f.

of the same composition as those employed in the previous Study to insure equality of tint for the experiences of hue under our conditions;[5] we did not allow for changes in chroma due to the film color.[6]

The procedure was the same as in the previous Study; but, as we were interested to find out something about the perceptive motive for the hue-response when there was a difference in tint, *O* sat, for the most part, at such a distance that he reported color only occasionally when color and screen were equal in tint. Owing to the difference in the limens of R and G and of B and Y, *O* reported the hue 'correctly' at every exposure of R and G at the nearer positions. At the start of every hour of observation we placed a disc of the no. 6 Hering grey on the mixer and set the resistance to give equality of tint to the two experiences. Despite this precaution we can not be sure that this particular resistance gave equal tints for the color and screen every time that we used it during the hour. The resistance steps were so large that the other brightnesses used would give, we supposed, a positive difference in tint so marked as to preclude any real effect on the reports of hue due to slight fluctuations. This supposition, in fact, proved incorrect; we return to the point later. But if a slight difference in tint had the effect of increasing the number of reports of hue, these slight fluctuations might and probably did result in a too high percentage of 'correct' reports of hue when the tints were supposed to be equal, and thus decreased the difference between the reports of hue when there was and, by hypothesis, was not a difference in tint.

The *O*s were Viola Don (D), a candidate for the A. M. degree, L. B. Hoisington (H), a member of the staff, and Dorothea Johannsen (J), a senior taking her major work in Psychology. D and J were not specially trained to make the kind of report called for.

The printed instructions were: "Fixate the center of the screen and keep the eyes as steady as possible. When the slide rises you may see a color; if you do see a color, report its hue." H, who observed after the others had completed the work, also reported on tint, especially if the tint of the center of the grey field (or of the color, when color appeared) differed but slightly from that of the surrounding screen. In this way we were able to check up on any difference in tint due to fluctuations of any kind. There was also

[5] *Ibid.*, 128.

[6] D. Ginsberg and L. B. Hoisington, The RL of Increased Chroma with Film Color, *Ibid.*, 35, 1924, 269 ff.

the additional advantage that we knew the color limens for this *O*, and hence could select the distances with greater assurance. As it happened, J likewise indicated the direction of tint-difference in 51% of her reports.

D sat at 3.25, 4.25, 5.25 and 6.25 m. from the screen; J at 1.25, 2.25 and 3.25 m.; H at 2, 3, 5 and 6 m. Even at these nearer positions J reported B 'correctly' but twice.

The results of the three *O*s differ very much as regards the number of reports of hue with the different brightnesses. The total numbers of reports of hue given by D for every stimulus and for every position at each one of the five intensities of brightness are, in the order from weak to strong, *i. e.*, from dark to light tint, 95, 113, 113, 142 and 171; by H, 182, 201, 97, 201, and 243; by J, 124, 115, 85, 114 and 143. The reports of hue given by D show no falling off with intensity III (if we refer to the intensities of brightness from weak to strong by the numerals I, II, III, IV and V) which, by hypothesis, gave equality of tint to the color and screen. Since she did not report on tint, we must for the moment let her results stand without comment.

The results of J yield considerably fewer reports of hue with brightness III than with the other brightnesses; but the difference is less than in the results of H. We have, fortunately, in her case, reports on other aspects of the experiences than hue; she reports 'dark,' 'light,' 'shiny,' 'no color' and 'nothing.' For the most part we may take her report of 'nothing' to mean that she saw no part of the grey field differentiated in any way whatsoever, and her report of 'no color' to mean the absence of hue although there was a darker or a lighter spot in the field. She usually reported 'shiny' with the lighter tints, and only occasionally with the darker; the term undoubtedly refers more particularly to the luminous character of the experience. A canvass of the reports of J shows that she gave the report 'nothing' 5 times with brightness II, 92 times with III, 91 times with IV and 4 times with V; that she reported 'dark' or 'light' 38 times with III and 26 times with IV; and that with the other brightnesses the number of reports of a difference of tint is much greater. The results indicate very clearly that, for this *O*, the simple precaution of equating the tints at the start of the hour was not sufficient; and we may conclude that it was, in like manner, not sufficient for D. Our previous work should, no doubt, have prepared us for this difficulty.

If we turn to the reports of H, where every experience was checked for difference of tint, we get undoubtedly a truer picture of the influence of difference of tint on the judgment of hue. If,

furthermore, we take the reports for those positions that were subliminal or nearly liminal for the particular color, we find hue reported 102, 122, 23, 121 and 163 times with the brightnesses I to V. The relatively small number of reports of hue when the tints were actually equal is a clear indication of the influence of difference of tint on the experience of hue.

To be sure, a great many of the experiences at the brightnesses above and below that which gave equality of tint were colorless; they differed from the background in tint only. Yet the number of reports of hue with the experiences which differed in tint is relatively so much greater than with those which did not that we must conclude that, for some reason, the very fact that the central portion of the field differed from its surroundings favored, although it did not compel, the appearance of hue.

We are further justified in this conclusion by the fact that, at the distances at which the stimulus was ordinarily or always inadequate to give an experience of hue, *O* often, if not usually, reported some other hue than that which the particular stimulus would normally arouse. Thus, at positions 3 and 4, D reported R and G 'falsely' 92 and 'correctly' 54 times; at all positions, she reported B and Y 'falsely' 120 and 'correctly' 103 times. H reported B and Y 'falsely' 145 and 'correctly' 159 times. J reported R and G 'falsely' 25 and 'correctly' 369 times; B and Y 'falsely' 125 and 'correctly' 62 times. The 'false' reports indicate that *O* read hue into the experience of tint-difference; for of the 150 'false' hues reported by H only one came when the tints were equal. Although the instruction admitted the possibility of hueless experiences, the *O*s, perhaps because hue did appear so often, seemed to have a 'set' for hue; this statement is especially true of D.

Furthermore, H reported: "I always look first for the hole in the screen. If I see it as dark or light it serves as something on which I can fixate." At another time he reported: "As soon as I see the dark or the light spot I look for its color, but I always look first for the spot." On one occasion J reported: "The hole looked like a piece of paper caught on the screen." At another time she said: "It is the same tint as the background, so I could hardly see it." Again she said: "There was something there; I could not tell what, as it was the same brightness." Experiences of hue, when of the same tint as the surrounding grey, melt into the grey; they are of the same texture; they have no boundaries; hence no meaning of object attaches to them. The presence of a tint-difference, on the other hand, results in a fairly well defined extent, and gives rise to the meaning of something different.

The relation of tint to hue, whether this is 'correctly' or 'falsely' reported, is further evidence of the fact that the hue was put on to the experience or read into the experience rather than correlated directly with the stimulus. When the tint was dark the *O*s 'falsely' reported the hue as R 89, as Y 2, as G 64, as B 46, as O 8, as P 25 and as BG 4 times; when the tint was light they 'falsely' reported R 28, Y 78, G 47, B 29, O 60, BG 13 and YG 6 times. These numbers do not include the reports with brightness III, which were R 37, Y 2, G 8, B 12, O 1 and BG 4. A dark tint, then, is likely to be seen as R, G, B or P; and a light tint as Y, O, BG or YG, although it may come as R, G or B. All *O*s saw lB as Y, and H saw dY as B, whereas the other *O*s saw it as R. There were other differences of the same nature between the reports of the *O*s; but it is obvious that tint suggested hue to a considerable degree.

A similar relation holds between tint and the 'correct' reports of hue: dR came 'correctly' 215 and lR 198 times; dY 22 and lY 183 times; dG 147 and lG 178 times; dB 5 and lB 40 times. These numbers, again, do not contain the reports with brightness III, which were for R 76, Y 12, G 46 and B 9. Again the reports of the separate *O*s yield differences; H, for example, saw lR 'correctly' more often than dR, and dG more often than lG, a result which is in harmony with his 'false' reports. It does not make very much difference to R and G whether they are darkened or lightened, or by how much; there is no marked or constant difference with brightnesses I and II or IV and V, although R tends to go a little higher with IV and G with V; either darkening or lightening, under our conditions, has the effect of enhancement. Although the numbers are small, it appears that to darken a B makes it slightly more effective; whereas to darken a Y nearly kills it, but to lighten it makes it almost compulsory. This result corresponds exactly with the distribution of 'false' hues that we found above.

Not only the fact but the very frequency of the 'false' reports (563 'false' of which D gave 263, H 150 and J 150, to 1183 'correct' of which D gave 371, H 381 and J 431) is additional proof that, when the determination of *O* is to expect color, and when some differentiated part of the field appears as something to which hue can attach, the *O* will very often see that differentiated part as colored, the particular hue depending, in part, upon its tint. Thus about 33% of all reports of hue did not correspond with the stimulus. How many of the 'correct' reports were touched off by the tint-experience we are unable to say; but if we consider the rela-

tion between tint and the reports of Y, for example, we can scarcely escape the conviction that a goodly number of them were so determined.

The fact that 1734 out of 3480 experiences were without hue shows that some other factor or group of factors helped to determine the experience. The *O*s vary widely in the relative number of their experiences of hue: D reported hue 634 and no hue 326 times; H reported hue 531, no hue 472, and nothing 197 times; J reported hue 581, no hue 547, and nothing 192 times. All we can say is that, given a film-experience that is darker or lighter than its surroundings, it is likely to arouse an experience of hue. Unfortunately we were unable in this Study to take further reports that might have brought out more precisely the basis or bases of the *O*s' judgments.

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THE TACTUAL PERCEPTION OF SIMPLE GEOMETRICAL FORMS*[1]

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The similarity of the attribute of extent in touch and in vision has led to the tacit assumption that objects should feel spatially as they look. We set out in the following experiment to obtain careful descriptions of the spatial patterns of several touch perceptions whose stimuli have definite visual patterns and to determine whether, as in the case of vision, tied images play any part in these patterns.

The perception of tactual form has been investigated from several angles. The first experimental work in the field is that of Major,[2] who determined the limens for four forms, angle, circle, disc, and solid triangle. The circle proved to be the most readily recognized, and the disc the least. Major followed a procedure with knowledge, and had his *O*s draw the forms as they were perceived. His table of subliminal judgments shows approximately the patterns that were reported when the *O*s did not recognize the forms. Among the studies most closely related to our problem is that by Bose and Kanji,[3] who required their *O*s to discriminate between solid forms applied in pairs to the forearm. They found that the perception of form was never in terms of touch alone but always involved other factors, *e. g.,* visual images. Zigler and Northup[4] attempted to determine the accuracy of judgments of tactual form, the magnitude of the stimulus necessary to give a form perception, and the psychological factors involved in such a perception. They, also, worked on the forearm and with solid forms. They conclude that "tactual form is not . . . . definitely and unfailingly apprehended;" that there are many cases "where salient topographical features of the tactual impression of a form are perceived in isolation . . . . that must be completed either in visual or tactual imagery or both;" and that "the stimuli of common geometrical figures must have a main dimension of 12-15 mm. to

*Received for publication by E. B. Titchener of the Editorial Board.

[1]The experimental work reported in this paper was begun in the Psychological Laboratory of the University of Michigan.

[2]D. R. Major, Cutaneous Perception of Form, *Amer. J. Psychol.,* 10, 1898, 143-147.

[3]S. K. Bose and N. L. Kanji, Perception of Form by Passive Touch, *Indian J. Psychol.,* 1, 1926, 93-101.

[4]M. J. Zigler and K. M. Northup, The Tactual Perception of Form, *Amer. J. Psychol.,* 37, 1926, 391-397.

give rise readily to tactual impressions of determinate shape if stimulation is made on the middle of the forearm." Zigler and Barrett[5] have extended this work with solid, outlined, and partially outlined figures in 5 sizes, presented on the ball of the thumb, the palm, and the volar surface of the forearm. They find that outlined figures are perceived more clearly than solid or pointed figures; that tactual tied images as well as visual images enter in to complete the perceptions from point stimuli; that the order of accuracy in perceiving forms is thumb, palm, forearm; that acute angles form the most striking features by which tactual forms are perceived; that positive perceptions of tactual form are relatively infrequent; and that many perceptions of tactual form pass through a definite temporal course.

The forms whose touch-patterns we investigated were the line, the circle, the equilateral triangle, and the square. We had two sizes and two types of stimuli. Sets A and B were outline-forms 15 mm. and 10 mm. respectively. Sets *a* and *b* were of the same sizes, but consisted only of the corner points of the figures. The thickness of the lines and the points was 1 mm. The stimuli were made of hard rubber mounted on wire handles and the edges and points were carefully rounded and smoothed. All were weighted to give the same pressure per square mm. (5 g.).

Stimulations were always made in the middle of the palm of the left hand. It was found in the preliminary work that the perceptual pattern for a given stimulus varied with the position of that stimulus on the hand; therefore every stimulus was set down always in the same position, *i. e.,* line, parallel to the longitudinal axis of the hand; triangle, vertex to the wrist; square, sides parallel to the axes of the hand. The *O*s were five undergraduates, J, E, L, R, and H, who had had no previous training in psychological observation; H. G. Scahill, S, and Dr. Claire C. Dimmick, D, who were trained *O*s. Before the regular experimental series, all *O*s were given practice in discriminating and describing various qualities and intensities of touch. For the stimuli in these preliminaries we used v. Frey hairs of 3 lengths, and blunt points of two sizes and two intensities.

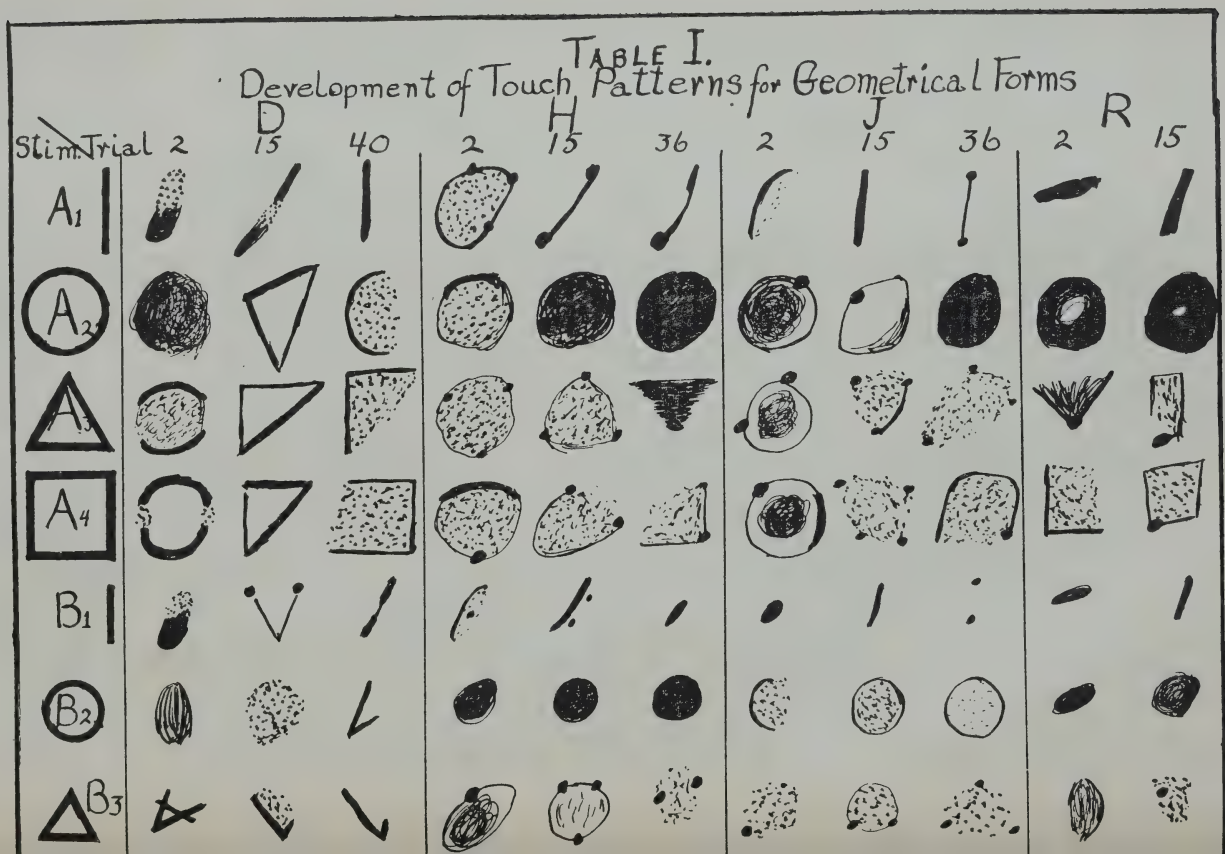
Procedure I

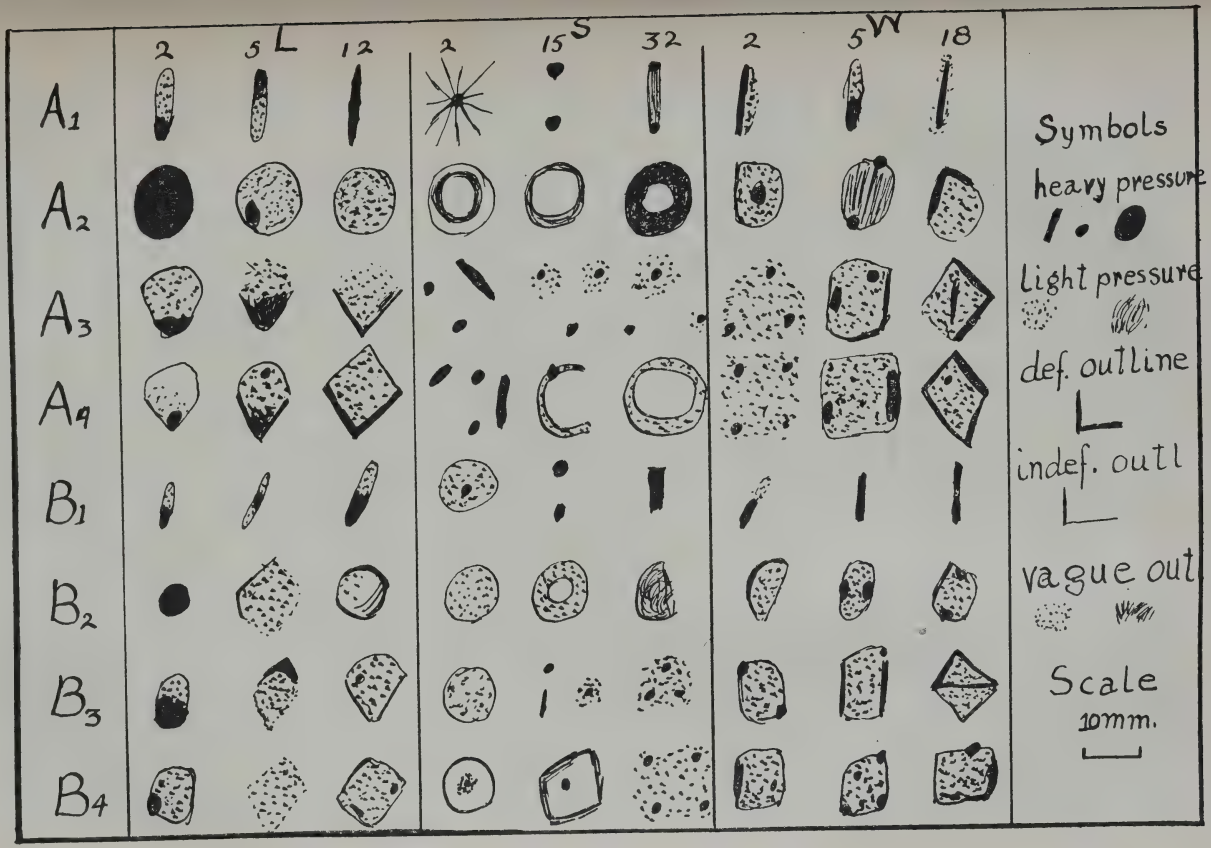
The *O*s were instructed: "A stimulus will be presented upon the palm of your hand that will arouse a tactual perception. Describe this perception as completely as you can. You may indi-

[5]M. J. Zigler and Rebecca Barrett, A Further Contribution to the Tactual Perception of Form, *J. Exper. Psychol.,* 9, 1926, 184-192.

TABLE I.

Development of Touch Patterns for Geometrical Forms




2
5 L
12
2
15 S
32
2
5 W
18
A1
A2
A3
A4
B1
B2
B3
B4
Symbols
heavy pressure
light pressure
def. outline
indef. outl
vague out
Scale
10mm.

cate the spatial pattern of the perception by a drawing if you wish. The stimulus will be repeated to complete the description." *O* sat with his left hand and forearm upon a table, palm up and with fingers slightly flexed. A screen was so placed that it concealed his hand and the stimuli from him. At a ready-signal *O* closed his eyes, and *E* set one of the stimuli down in its proper position upon *O*'s palm and held it there for 2 sec. As soon as he removed it *O* gave his description of the experience and made his sketch of the pattern. *E* repeated the presentation twice more with the same stimulus. After a short rest he continued with another stimulus, and so on. In this way every stimulus was presented to every *O* 15 to 40 times. Only the completely outlined forms, sets A and B, were given under this procedure.

In Table I we have reproduced the drawings of the several *O*s representing the tactual patterns set up by the different stimuli at successive stages in the development of the perceptions. The first fact that stands out, both in the verbal reports and in the sketches, is that at first there were no perceptions of form in the usual (visual) sense of the term. The experiences were pressures variously extended and differing in quality and intensity in various parts. To most of the *O*s all of the experiences at first were more or less circular extents—not circles. At this stage the areas usually were completely filled in tactually. There were no lines, though points of more intense pressure occurred even with the circular stimuli. However, although perceptions of form were not experienced immediately, the patterns of the perceptions which were vague and indefinite at first soon became fairly exact and consistent.

The first shape that came to be definitely formed was the line. The longer line began to assume its appropriate shape after as few as 5 or 6 presentations with some *O*s (J, R). As a rule the short line was not far behind. Often the tactual patterns were curved or had appendages; but they were definitely lines. As a final stage in the case of 2 *O*s (J, H) the outstanding characteristics of the lines were the end-parts. Frequently the mid-portions went unperceived; *i. e.,* the pattern consisted of 2 disconnected parts of fairly intense pressure which, however, carried the meaning of line.

The perception of 'circle' was established almost as rapidly and as definitely as that of 'line'. However, two *O*s (W, D) had difficulty with the circle and never perceived it as such. Neither of the other two forms, triangle and square, developed adequate 'form'-perceptions. The patterns for both the small and large

stimuli remained apparently undefined extents of pressure of varying intensities and qualities.

In all of these observations the patterns were almost totally tactual. Very little visual imagery entered in (despite the fact that the *O*s invariably translated the tactual patterns into visual patterns in their reports), and when it occurred it bore no reference to the stimulus-object and did not tend to give form to the tactual elements.

We may say, then, that the purely tactual perception of form does not occur spontaneously, though with repetition perception of the simplest forms may be built up.

Procedure II

Although the tactual patterns appeared to be quite indefinite, bore little resemblance to the shape of the stimuli, and differed remarkably from *O* to *O*, the pattern for any stimulus became consistently established for a particular *O*. Our second problem was to determine how consistent these purely tactual patterns were. The *O*s were now instructed: "A series of 8 stimuli will be presented on the palm of your left hand. Every one will be designated by a symbol, *e. g.*, A1, B1, A2, B2, etc. You are to associate the symbol with the experience. After two practice series, the stimuli will be presented in haphazard order and you will be required to name them with their appropriate symbols. Further practice will be given until you are able to name all of them correctly." Three *O*s, (D, J, R) served in these series.

TABLE II

Stability of the Perceptual Patterns

O	% correct after 1 practice series	No. of practice series to attain 100%	% correct for the 4 forms in all series							
			A1	A2	A3	A4	B1	B2	B3	B4
D	50	5	90	50	80	100	100	60	60	50
J	88	2	80	80	100	100	80	90	63	50
R	77	3	83	58	67	100	80	50	100	67
		Average	84	63	82	100	87	67	74	56
		M. V.	4—	12—	12—	0	9	16—	17	8—

From Table II it is evident that although the tactual patterns given in Table I resemble only vaguely the visual 'forms' which we are accustomed to associate with their respective stimuli, they are relatively stable and readily become concrete perceptual experiences.[6] All *O*s had had the practice of Procedure I, which called for analytical descriptions, and when their task was shifted

[6]Friedline has shown that this result holds for 2-point stimuli which differ as little as 5 mm. *Amer. J. Psychol.*, 29, 1918, 400-419.

to that of recognizing the several perceptual patterns, they very soon attained perfect accuracy.

A second important fact is that the percentages of correctness for the various stimuli do not follow the order of the establishment of the perception of forms found by Procedure I. The lines, which were the first to give rise to the perceptions of definite 'forms,' were the most correctly recognized; but the second 'form' to develop, the circle, was the most difficult to recognize. On the other hand, the triangles and the squares, which did not become 'forms,' gave definite and easily recognizable tactual patterns, with the exception of the smaller square, A re-examination of the tactual patterns of Table I shows that although the triangles and squares did not give rise to triangular and square experiences, their respective 'feels' were more distinctive than those from the circular stimuli.[7] This fact argues that the absence of a correlation between visual and tactual patterns does not mean that the tactual experiences are indefinite or inadequate.

Procedure III: Tactual Tied Images

The presence of a complete tactual filling-in of the entire area covered by a stimulus which consisted merely of an outline gave rise to the question whether tactual tied images are commonly present and, if they are, what are their conditions and qualitative characteristics.

Our understanding of 'tied image' is based on the descriptions given in the fields of vision[8] and of audition.[9] They occur under the "object consciousness" and consist of qualitative-spatial or—temporal additions to an incomplete perceptual presentation. They differ from 'memory color'[10] images in that the latter are primarily qualitative overlays, whereas 'tied images' are primarily temporal or spatial completions.

It may appear, at first glance, that the filling-in of the various perceptions that is evident in Table I may be classed under 'tied image.' Such an interpretation seems to us to be unjustified. Our *O*s were all under a descriptive attitude, not under a stimulus at-

[7]It is probable that Zigler's *O*s, being set to perceive forms similar to the visual forms, reported in terms of the correspondence between their tactual patterns and the visual concepts rather than in terms of the distinctness of the tactual experiences, and so attained only a low degree of accuracy.

[8]M. J. Zigler, An Experimental Study of Visual Form, *Amer. J. Psychol.*, 30, 1920, 273.

[9]F. L. Dimmick, An Experimental Study of Auditory Tied-images, *ibid.*, 34, 1923, 85.

[10]G. K. Adams, An Experimental Study of Memory Color and Related Phenomena, *ibid.*, 34, 1923, 359.

titude. They were not dealing with known objects; and the 'filled-in' portions were as much a part of the total pattern directly referable to the stimuli as were the corners, edges, etc. when they occurred. It is probable that the filling-in is a matter of the excitation set up by the particular stimuli.

In order to call out true tied images we presented the stimuli of sets *a* and *b*, the point stimuli, under the same instructions as in Procedure I, on the assumption that if, in the preceding work, definite perceptual patterns had been built up, these new stimuli might be mistaken for the old stimuli (A and B) and the missing parts be filled in with imagery.

TABLE III

FREQUENCY OF TACTUAL SPATIAL ADDITIONS

O	% complete	% partial	% no filling
D	30	64	6
J	60	31	9
R	16	36	48

"Complete" means that the entire outline of the figure was perceived; "partial", that some of the outline was perceived or that the figure was in part solid; "no filling", that only a number of points was felt.

The results in Table III indicate that our conditions were adequate for the setting up of tied images of touch. Early in the series many of the descriptions and descriptive sketches showed patterns identical with those given for the completely outlined stimuli. As the series progressed, however, it became evident that the perceptual patterns set up in the first series were losing their effect, and eventually the tied images disappeared completely. At this stage all of the point stimuli gave point patterns with the exception of the small 2-point figure (b1) which continued to be felt frequently as a line.

Along with the appearance of these new tactual patterns, came the application of geometrical names to them. The points were the distinguishing characteristics of the various 'forms,' which were given their complete meaning in terms of visual images. After the completion of the series for tied images the original A and B sets were presented again under the previous instructions. All of their patterns were modified by the work with the point stimuli. Perceptions of 'form' became the rule; the patterns were reduced to points of pressure with no intervening tactual experience reportable; and visual images of the 'forms' gave the meanings to the perceptions.[11]

[11]Cf. Zigler and Northup, *op. cit.*

Conclusions

Our experimental work shows (1) that purely tactual perceptions do not naturally take on the meanings of form that correspond with our visual geometrical forms; (2) that when such tactual perceptions of form are built up, it is only by the addition of other elements, usually visual; (3) that in spite of their lack of resemblance to the visual forms of the stimuli the tactual patterns are stable and readily recognized; and (4) that tied images, similar to those in vision and audition, play a large part under the right conditions in filling out the tactual patterns from incomplete stimuli.

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THE MEASUREMENT OF RETINAL FATIGUE TO ACHROMATIC STIMULATION—I*

FRANK ARTHUR GELDARD

INTRODUCTORY

It is well known that continued stimulation of the retina results in a changed sensitivity of the photosensitive elements of the eye. Where stimulation is chromatic this sensitivity change is manifested in a reduction in seen luminosity, chroma, and possibly a changed color-tone. In the case of achromatic stimulation variation can take place in one direction only and the result of such stimulation manifests itself in a reduction in seen luminosity. These facts constitute a simple statement of the nature of retinal adaptation or fatigue. The term "adaptation" has received a variety of usages as has the word "fatigue". The term "fatigue" is here employed in the special sense demanded by its usage in current visual psychophysiology. When used in this way it is quite free from any theoretical implications.

It would seem that an entirely adequate description of retinal fatigue must be centered about a quantitative determination of the course of the process. Such a determination will be of interest not only for itself but also for its value in our consideration of visual theory in general, the factor of fatigue being most elemental in that connection. It is the aim of the present paper to give a quantitative account of the process entailed in retinal fatigue to white light. The nature of certain problems growing out of the method here employed will also be indicated.

HISTORICAL

The first thorough investigation of the fatigue process was made fifty years ago by von Kries.[1] It is true that C. F. Müller[2] some years before had made an attempt at the determination of the nature of the fatigue curves, but that his method was faulty is evidenced by the criticism proffered by von Kries. Müller had emphasized his finding that the course of fatigue is in no way depend-

*Received for publication by John Paul Nafe of the Editorial Board. This work was originally reported at the meeting of the Experimentalists at Harvard in April, 1927.

[1]J. von Kries, *Ueber Ermüdung des Sehnerven.* Graefe's Arch. f. Ophth., 23, (2), 1877, 1.

[2]C. F. Müller, *Ueber den zeitlichen Verlauf der Netzhaut Ermüdung.* Dissertation, Zurich, 1866.

ent upon the absolute intensity of the fatiguing light. It was pointed out by von Kries that Müller could have no certain knowledge on this point in view of the fact that the range of intensities employed was altogether too small for the detection of differences.

The work of von Kries is admirable in view of the facilities at hand at the time it was performed. Briefly, his method was as follows: A compound disc composed of an inner disc of white paper and an outer ring of black and white sectors was revolved on a hand-propelled color-mixer. The inner disc constituted the fatiguing area; the outer section was used as a comparison area. Illumination was secured by an oil lamp. The light was caused to fall upon the paper surfaces and reflected light from these directly stimulated the eye. Observation was monocular and the common center of the two discs served as a fixation point. The central area of the retina was first fatigued for a definite period by the reflected light from the inner disc. During this period a black shield was interposed between the eye and the outer ring. At the end of the fatiguing period the shield was removed and the outer ring was matched with the inner for brightness. The outer ring was of course distinctly peripheral as contrasted with the inner surface. The ratio between the intensities of the two areas then gave a measure of the fatigue that had been induced in the central area. By appropriately varying the time of fatigue the process could be broken off at any point and cross-section measurement made of it, so to speak. By changing the distance between the lamp and the disc the process could be measured with respect to dependence upon the intensity of the fatiguing light. There are obvious defects in the method, especially with respect to failure to isolate the rôle of the pupil. They will be spoken of later. A summary of von Kries' results are reproduced in Table I.[3]

TABLE I

SUMMARY OF THE RESULTS OF VON KRIES' EXPERIMENT

Relative light-intensity	Apparent decrease after:							
	3	6	10	20	40	80	160	secs.
1	.91	.81	.66	.58	.43	.23	.15	
1.95	.86	.74	.62	.52	.32	.18	.09	
3.9	.82	.71	.62	.34	.21	.14	.08	
34.7	.74	.57	.42	.25	.16	.08	.03	

Removing the decimals, the values presented in this table may

[3] J. von Kries, Nagel's Handbuch der Physiologie der Menschen, III, 216. The same data are treated graphically in the original work, op. cit., p. 16.

be read as percentages of the original stimulus light apparently remaining after definite periods of fatigue. The values were computed on the basis of 1.00 as the value of the stimulus light at each intensity. Since, theoretically, the area of stimulation remains the same throughout, we may consider the values to be representative of the amounts of remaining apparent brightness, although the figures were derived from the computation of the relative physical intensities of the lights used.

With a presentation of the work of von Kries our consideration of the literature of the problem is nearly complete, for, whereas the phenomenon of color fatigue has received a variety of treatments with respect to diminution in saturation, brightness, and hue change, the problem of the nature of fatigue to white light has received scanty consideration since von Kries' work.

Another type of research involving the measurement of the effects of stimulation may be mentioned in order that a clearer conception may be obtained of the action of light energy upon the retina. This involves the measurement of the so-called "action time" of the retina. When light is caused to impinge on the retina a certain length of time must elapse before the arousal of the most intense sensation that the stimulus is capable of exciting. The least time that is required for this rise in intensity to reach a maximum is called the "action time" of the retina. This interval, though of very brief duration, is capable of measurement. McDougall[4] concludes with regard to the course of the rise in sensation intensity that the intensity of sensation varies directly with the duration of the action of the light. It has further been shown that the rate of rise varies with the stimulus intensity and that the action-time diminishes with increase in the physical intensity of the stimulus. The results of prolonged stimulation may then be regarded as a compounding of the two effects, a rapid rise in sensation intensity followed by a relatively slow decrease in the same. If the process were represented graphically, the curve of sensation intensity as measured in brightness values, plotted against time, would show a precipitous initial rise, then a relatively slow decline. The work here reported deals only with the second part of the curve and tacitly assumes the dependence of the first sudden rise on the intensity of the stimulus.

Apparatus and Method

The method employed in this experiment has certain points in

[4]McDougall, W., The Variation of the Intensity of Visual Sensation with the Duration of the Stimulus, *Brit. J. Psychol.*, 1, 1904, 151.

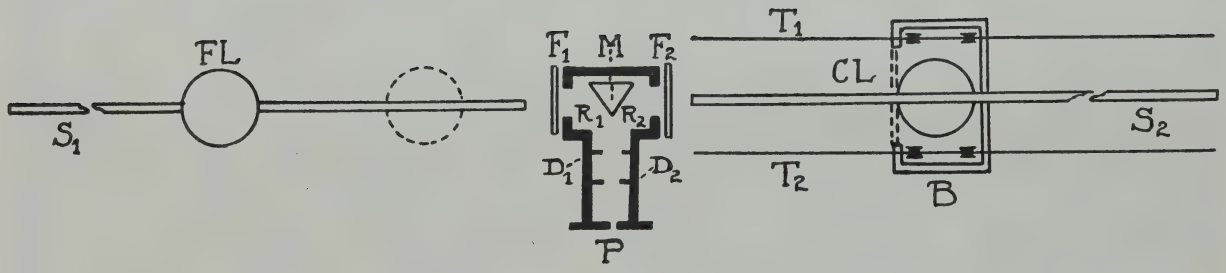

FIGURE I

SCHEMATIC DIAGRAM OF THE APPARATUS—TOP VIEW

FL, fatiguing light, 400 watt daylight lamp; CL, comparison light, 250 watt daylight lamp; B, box carrying CL; S_1 and S_2, graduated scales; M, magnesium carbonate block; R_1 and R_2, reflecting surfaces; D_1 and D_2, diaphragms; P, artificial pupil; T_1 and T_2, trolley wires carrying 110v. A. C.; F_1 and F_2, ground glass filters.

common with that used by von Kries. There are also some fundamental differences between the two methods. Some of these differences will be apparent from a description of the apparatus which is schematically represented in Figure I. The apparatus as diagrammed consists essentially of a modified photometer and two daylight lamps, one on either side of the photometer, the intensities of which may be altered at will by the distance method. The lamp on the left side of the apparatus is a fatiguing light (FL). It is a 400 watt daylight projection lamp mounted on a graduated scale (S_1). The scale itself is 165.8 cm. long. The light to the right of the photometer is a 250 watt daylight projection lamp and serves as a comparison light (CL). It is mounted in a wooden box (B) which is itself suspended by small pulleys to two bare trolley wires conducting a 110 volt A. C. current. The lamp is wired directly to the pulleys. The weight of the box and the fact that each of the two lamp terminals is wired to two pulleys insure good contacts at all times. A pointer attached to the top of the box lies along a centimeter scale (S_2), 167.6 cm. in length. The position of the box may be changed by the manipulation of a spool connected with an endless pulley arrangement which is likewise connected with the box. The spool is situated at the observer's right hand and may be moved either by the experimenter or by the observer when under instruction to do so. The structure of the photometer itself is apparent. The reflecting areas are surfaces of a magnesium carbonate triangular prism (M). There are two adjustable diaphragms (D_1 and D_2) in the optical system. These could regulate the size of the field, but in these experiments the field was kept at a constant diameter. The stimulus field consisted of the two reflecting surfaces and appeared to the observers as a flat circular disc bisected by a narrow black line, (the near edge of the prism). The left half of the field was illuminated by the fatiguing light, and the right half by the comparison light. A tiny point bisected the dividing line and served as a fixation point. The entire field subtended a visual angle of 6°47′. The ocular opening (P) is very small, 2.2 mm. in diameter, and serves as an artificial pupil. Inasmuch as the natural pupil of the eye fluctuates in diameter even when the conditions of illumination are held constant, the amount of light energy impinging on the retina can be made uniform in amount only by restricting, by some such device, the amount of light entering the eye. An excellent discussion of the theory underlying the use of the artificial pupil is set forth in a paper by Troland[5]. Ground glass filters (F_1 and F_2), having

[5]Troland, L. T., The Theory and Practice of the Artificial Pupil, *Psychol. Rev.*, **22**, 1915, 167-177.

transmission values of .42, are placed in the path from the stimulus lights to the reflecting surfaces in order that a sufficiently large range of intensities can be supplied by the fatiguing light. The lower intensities are produced with the aid of these filters; for the higher intensities the filters are removed. The entire apparatus is set up in a dark room and light from one side of the apparatus is prevented from entering into the opposite side by appropriately placed black velvet screens. The light circuits are controlled from a faintly illuminated switchboard at the opposite side of the dark room some distance away from the apparatus proper and at the observer's back. A stop-watch is likewise conveniently mounted on the switchboard.

The following observers served in these experiments: Dr. J. P. Nafe (N), under whose direction the work was performed, J. D. Bryden (B), and F. A. Geldard (G), the last two being graduate students in this department. C. H. Graham, likewise a student in the department, kindly acted as experimenter in those series in which the writer served as observer.

As a preliminary step in the method each observer was required to equate the brightnesses of the two halves of the field. *O* was instructed in the manner of securing registration of the pupil and in manipulating the spool in such a way as to bring the comparison light in a position of equal energy with the fatiguing light. Both lights were presented simultaneously and *O* was allowed ten seconds to effect a match. These preliminaries were continued for three observation periods of 45 minutes each. For three more such periods the procedure was changed to the conditions of the later experiments. *O* was instructed as follows: "You will be given a "ready-now" signal. At the "ready" take your position at the ocular. You will have been previously informed as to which eye is to be used in a particular trial. At the "now" the left half of the stimulus field will be illuminated. You are to fixate immediately the point in the middle of the median line. This fixation must be maintained strictly throughout the course of each trial. At a second "ready-now" signal the other half of the stimulus field will be illuminated. Maintaining fixation you are to pay particular attention to the brightness of the right half of the field as contrasted with the left half. I shall count from one to ten. Immediately following the count of ten the lights will be extinguished and you are to report what the brightness conditions were *at the count of ten.* Your report will always be with reference to the right half of the field, that is, it is brighter than, less bright than, or equal in brightness to the left side of the field."

A word must be said concerning the matter of allowing ten seconds for the match to be made. It is evident from the description of the procedure thus far given that the method consisted essentially of fatiguing one area in the retina with a constant light of known intensity, then obtaining a measure of the reduction in brightness by the introduction of the comparison light which stimulated an area equal and adjacent to the fatigued area. The stimulation from the comparison light, however, constitutes a new fatigue. At best, then, the method consists in the measurement of the difference between two "fatigues". This is a most important point. The choice of the ten second match was made after the trial of longer and shorter periods of matching. Inasmuch as the drop in brightness is very rapid in the first few seconds of fatigue, the report of the observer would be less reliable under the conditions of a very short period of comparison as contrasted with a relatively long period. The comparison of shorter duration would, of course, be more desirable in that the derived values would be closer to a true measure of the absolute amount of fatigue present at the time of matching. The ten second interval was selected mainly for the reason that a more stable condition of the comparison field is present under those circumstances. The briefer interval does not remain an impossibility, however.

Three different intensities of fatiguing light were used. The lowest intensity had a value of 69.0 apparent candle-meters. By apparent candle-meter is meant the intensity of one candle-meter reflected from the surface of the magnesium carbonate block. The highest intensity had a value of 13,805.4 a. c. m. The third intensity used was intermediate and had an absolute value of 690.3 a. c. m. It will be seen that the intensities vary in the proportion of 1: 10: 200. When it is considered that the eye is capable of stimulation by light intensities covering a range of some forty million to one, the range of intensities used in this work does not appear to be very great. It is, however, great enough for the detection of any differential effect of the change of intensity.

Before any series of exposures the observer's eyes were thoroughly dark-adapted. Since the area of the retina stimulated was not strictly foveal but extended just outside the macula, a period of approximately five minutes was allowed for dark adaptation to occur. In accordance with the findings of Hecht[6] [7] the sensitivity

[6]Hecht, S., The Nature of Foveal Dark Adaptation, *J. Gen. Physiol.*, **4**, 1921-2, 113.

[7]Hecht, S., Dark Adaptation of the Human Eye, *J. Gen. Physiol.*, **2**, 1919-20, 499.

curve should have reached an approximate level in that time. Since the eyes were stimulated alternately, sufficient time for recovery elapsed between successive exposures.

It was necessary before giving a fatigue series to determine the value of the comparison light, in terms of a scale reading, that was equivalent to the fatiguing light value. Accordingly, a series of ten rapid matches was made by the observer. The average of these readings was taken as the desired value. The nature of the fatigue experiments that followed is evident from the instructions. The left half of the field was presented for a period of 5, 10, 15, 20, 30, 45, 60, 90, or 120 seconds after which the comparison field was illuminated and a report of the brightness conditions was made after a ten second match. The comparison light was set by the experimenter at a guessed value for the first observation of a series at any given time and intensity of fatiguing light. The subsequent settings were largely guided by the reports of the observer. The number of "equal" reports that were required to fix a value

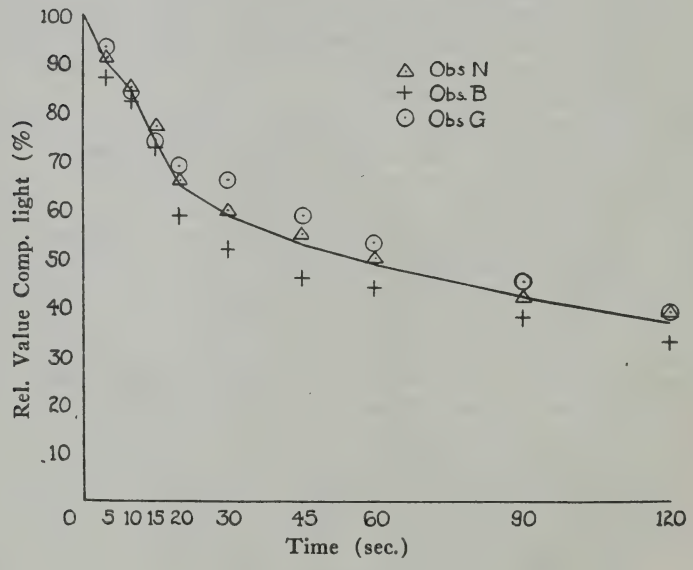

FIGURE II
FATIGUE CURVE FOR INT. 1

varied, the number of trials being dependent upon the consistency of the reports. The usual number was ten, however.

Results and Discussion

The results of the experiment are best indicated in tabular and graphical form. Table II gives for Intensity I, the values representative of the physical intensities of the comparison light, computed on the basis of 1.00 as the value under conditions of no fatigue. The individual measures for each observer as well as the average of the three are given. The same data are presented

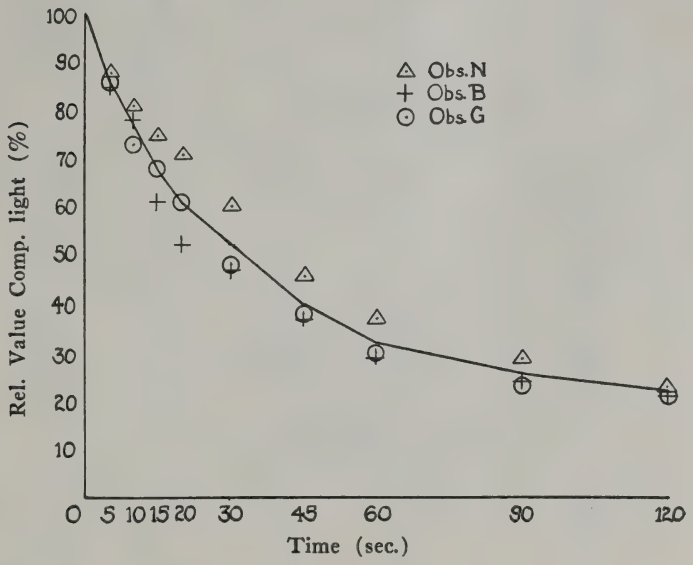

FIGURE III
Fatigue Curve for Int. 10

graphically in Figure II. There the relative values of the comparison light are put in terms of percentages of the "no-fatigue" value. The individual records are indicated and the average curve is plotted with time on the abscissae and the relative value of the comparison light on the ordinates. Table III and Figure III give the data gathered from the measurements with Intensity 10 of the fatiguing light. In the same way Table IV and Figure IV present the results for Intensity 200. As has been indicated the individual values of the tables are made up of the averages of a series of measures. The probable errors of these values range from 0 to $\pm$ 1.01. The deviations from the mean of the measures lying on the curves are evident from the distribution of the individual values

TABLE II

SUMMARY OF RESULTS FOR INT. 1

Fatigue period (Sec.)	Obs. N	Obs. B	Obs. G	Average
5	.91	.87	.93	.90
10	.85	.82	.84	.84
15	.77	.73	.73	.74
20	.66	.59	.69	.65
30	.60	.52	.66	.59
45	.55	.46	.59	.53
60	.50	.44	.53	.49
90	.42	.38	.45	.42
120	.39	.33	.39	.37

TABLE III

SUMMARY OF RESULTS FOR INT. 10

Fatigue period (Sec.)	Obs. N	Obs. B	Obs. G	Average
5	.88	.85	.86	.86
10	.81	.78	.73	.77
15	.75	.61	.68	.68
20	.71	.52	.61	.61
30	.60	.47	.48	.52
45	.46	.37	.38	.40
60	.37	.29	.30	.32
90	.29	.24	.23	.25
120	.23	.21	.21	.22

TABLE IV

SUMMARY OF RESULTS FOR INT. 200

Fatigue period (Sec.)	Obs. N	Obs. B	Obs. G	Average
5	.71	.62	.75	.69
10	.57	.57	.66	.60
15	.39	.42	.52	.44
20	.35	.31	.38	.35
30	.28	.25	.28	.27
45	.18	.18	.21	.19
60	.13	.13	.15	.14
90	.10	.11	.12	.11
120	.10	.09	.10	.10

plotted on the graphs. Whereas a comparison of three observers is of little value in the detection of individual differences with respect to rate of fatigue, it is rather noticeable that the measures derived from those series in which B served as observer are quite uniformly low with respect to the values for observers N and G. It is entirely possible that there exist marked individual variations with respect to "fatiguability" of the eyes. It is likewise possible that some such factor as visual acuity may correlate significantly with the rate of fatigue. This is, of course, purely conjec-

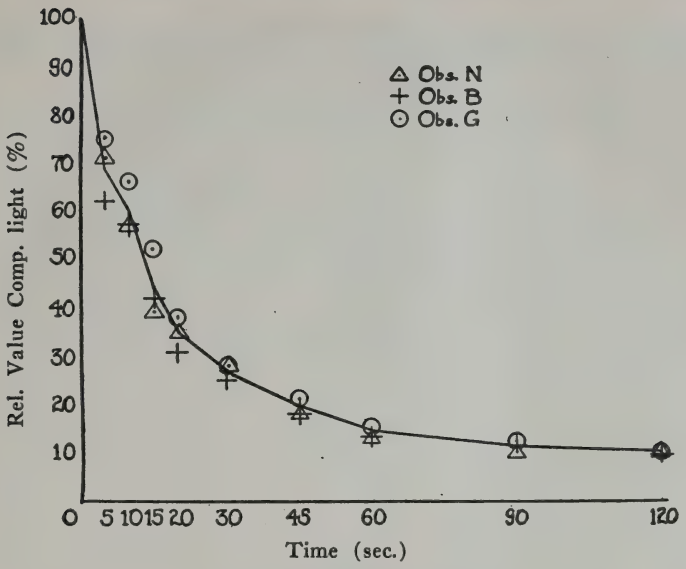

FIGURE IV
FATIGUE CURVE FOR INT. 200

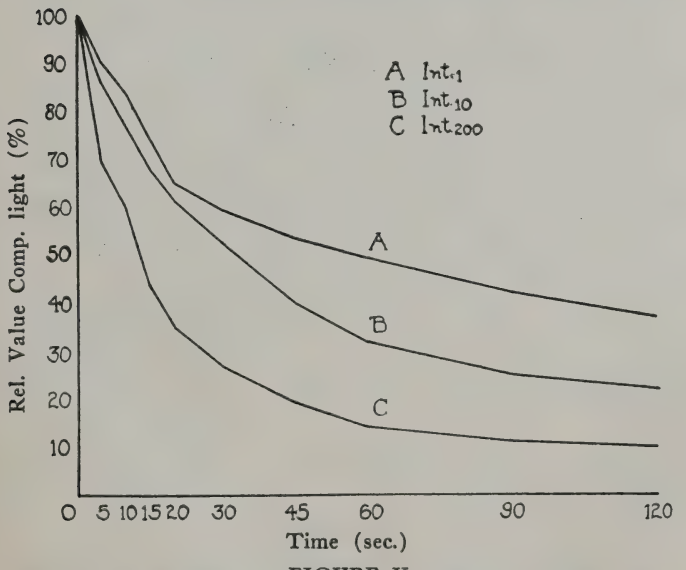

FIGURE V
COMPARATIVE FATIGUE CURVES FOR THE THREE INTENSITIES USED

tural, but the relationship suggests itself as a possibility. Figure V gives an interesting graphic comparative analysis of the course of the fatigue process at the three intensities.

It must be remembered that the values presented in the tables are indicative of the difference of the effects of two different periods of stimulation, or, as has been stated before, the difference between two "fatigues". The question may be raised as to whether the comparison period may be considered as a constant factor in the experiment and a constant correction for it be made in order to derive the values for the absolute amount of fatigue. The answer to this question hinges upon the solution of another specific problem. This problem may be stated as follows: Are the effects of stimulation of one part of the retina transferable to other parts of the retina? If stimulation of one area does not affect any other part of the retina, then such a methodological device as was used in these experiments is entirely valid and the match as made under these conditions can be simply evaluated. Such an assumption has been quite generally made in the older literature. Voeste,[8] for example, in his experiments on hue changes in color fatigue takes the comparison made by one area of the retina to be a reliable measure of the fatigue induced in an adjacent area. The same question was raised by von Kries.[9] His opinion was that the mutual effect of one area upon another may me considered negligible. Of late, however, the question has been seriously raised as to whether there may not be some such transfer effect. Hecht[10] assumes that when "two contiguous parts of the retina are illuminated one by I and the other by $I+\Delta I$ the same results obtain as when the same area is subjected successively to these just discriminable intensities." Cobb,[11] in his work on the effects of background on brightness discrimination, has adduced evidence to show that an area of the retina surrounding an illuminated area cannot be regarded as being uninfluenced by the stimulation of the illuminated area. The best evidence seems to point to an interdependence of retinal parts.

A word may be said with respect to the extent of this influence in these experiments. In view of the fact that the comparison area was immediately adjacent to the fatigue area on one surface only,

[8]Voeste, H. Messende Versuche über die Qualitätsänderungen der Spectralfarben in Folge von Ermüdung der Netzhaut, *Z. f. Psych. u. Physiol. d. Sinn.*, 43, 1896, 258.

[9]Op. cit.

[10]Hecht, S. Visual Intensity Discrimination, *J. Gen. Physiol.*, 7, 1924, 253.

[11]Cobb, P. W. The Effect on Foveal Vision of Bright Surroundings, *J. Exper. Psychol.*, 1, 1916, 540.

the median line, it is extremely unlikely that the interaction of parts would be as great as in the von Kries' experiments. It will be recalled that the two areas in his work were concentric. The circumference of the inner disc formed a relatively large surface as contrasted with the line of division used here. Undoubtedly, the best check on the hypothesis that there is an interaction will be obtained from a binocular match. The validity of such a method of matching rests upon the problem of the dependence or independence of the retinae. The usual assumption is that they are independent. A large body of literature has been directed at the solution of this problem. A consideration of the arguments pro and con will be included in a later paper.

No attempt has been made in this paper to express mathematically the relationship between time and intensity in the determination of the course of fatigue. That there is a marked dependence of the process upon both factors is plainly evident from a comparison of the fatigue curves. It is the plan of the author to experiment with a great enough range of intensities to enable him to determine accurately the nature of the relationship. It is further planned to arrive at a proper evaluation of the matching method employed in this experiment by the use of other lines of approach to the problem.

Summary

The nature of the effect of prolonged stimulation of the retina by white light has been discussed from the historical standpoint. The relationship between this type of work and the measurement of the "action-time" of the retina has been indicated. An apparatus designed to measure retinal fatigue has been described and a method of investigating the course of the fatigue process has been outlined. Using this method the author has been able to make a series of measurements of the decrease in seen brightness accompanying prolonged achromatic stimulation. It has been found that the process is dependent upon both the time of stimulation and the intensity of the fatiguing light. Certain problems growing out of the method here used have been discussed and the plans for future investigation have been indicated.

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A PHENOMENOLOGICAL STUDY OF LUSTER[1]

F. Lovell Bixby

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Introduction

The object of the following paper is to describe the phenomenon of luster. We say 'phenomenon,' rather than 'phenomena,' because it turns out that luster is always and everywhere the same, and that the various 'kinds' of luster distinguished by mineralogists and in ordinary speech are in reality various modes of appearance, not of luster itself, but of the surfaces, of lustrous objects.

The experiments may be considered in five groups: (A) preliminary experiments, in which we attempted a first rough characterization of luster; (B) experiments with naturally lustrous objects; (C) experiments with synthetized luster; (D) experiments undertaken, in continuation of the second group, to determine the phenomenological basis of the modal differences ordinarily attributed to luster; and (E) miscellaneous experiments (stereoscopic luster, casual observations of 'reduced' luster, and observa-

[1]From the Psychological Laboratory of Cornell University: transmitted by E. B. Titchener, H. P. Weld and L. B. Hoisington.

tions of non-lustrous surfaces which give the meaning of luster).[2]

Instructions. Throughout the investigation, except in the experiments of the fourth group, the instructions remained the same but for one word. For the experiments of the third group they read: "You will be given a visual experience. I wish you to give a phenomenological description of the experience." For those of the first and second groups they read: "You will be given a visual object. I wish you to give a phenomenological description of the object."

Observers. Our *O*s were Dr. L. B. Hoisington (H), assistant professor of psychology; Dr. S. Feldman (F), instructor in psychology; Mr. J. P. Guilford (G), and Mr. G. Kreezer (K), assistants in psychology; Dr. E. F. Moeller (M), honorary fellow in psychology; Miss Florence Woolsey (W), Miss Lillian Hatfield (Ha), and Mr. H. D. Powers (P), graduate students in the department; and Mr. F. L. Bixby (B), who was also the *E*. H, F, and M had had previous experience in phenomenological observation. H and M became acquainted, in the preliminary experiments, with the general nature of the problem; M observed only in these preliminary experiments. In the experiments of the fourth group (on classification) H alone served as *O*. B is responsible for most of the observations in the fifth group, except for those on stereoscopic luster, in which H also took part.

(A) Preliminary Experiments

In this group of experiments we used as stimuli three typically lustrous objects: a piece of black taffeta 15 cm square; a piece of polished zinc 12 x 10 cm; and a shell (Unio verrucosus) that gave a lustrous surface roughly 7 cm square. At first the stimulus was placed on a table in a dark room, from the ceiling of which hung a 100-watt mazda daylight lamp. The *O* sat with his arms resting on the table, and leaned forward for observation. The light was swung to various distances and in different directions until the *O* declared the conditions favorable for the observation of luster. The light was finally fixed at a height of 150 cm above the stimulus-object, and at a distance of 75 cm in front and 50 cm to the right. The stimulus, except in the case of the shell, was now covered with a sheet of grey cardboard having a circular orifice 4.5 cm in diam. through which the used area of the stimulus-object appeared.

We performed 10 experiments with each *O* to find out the best conditions of illumination, and to get a rough characterization of luster as a visual phenomenon. (1) Our two trained *O*s agreed, within very narrow limits, in the placing of the light. The selected arrangement gave a fairly uniform field, without strong high lights, by no means dull and yet not brilliant. (2) Luster was

[2]The order of exposition does not, in all cases, represent the order of experimentation.

characterized as a 'glowy', 'luminous' light coming from 'inside' the object.

Preliminary Characterization of Luster

(H, Zinc, 1) There is a layer of light that I can't say much about. It seems to play through the dark. It is not like reflected light but like transmitted brightness. (H, Black Taffeta, 4) There are two definite strips of a brighter luster, and in those two areas there is undoubtedly some sort of brightness that is localized beyond the black. It is not the brightness of a surface, but like the glow of a just hidden light. (H, Shell, 5) Where I see the luster is in the shift from high-light to dull. The luster is not right out in front; there is something that gives the meaning of a very tenuous surface, a very thin veil, and all the luster is behind that. What is seen through this veil is bright with a luminous-like brightness.

(M, Black Taffeta, 2) The shine does not seem to come from the surface but seems to be a glow that comes from the inside out. (M, Zinc, 6) The surface is streaked with alternate light and dark points, like pin-points, and it is as if there was a light behind and shining through. (M, Zinc, 8) There is something glowy about the lustrous parts which seems to come from away back. (M, Shell, 9) The luster is back behind something which is like a very highly polished glass; it is glowy, the kind of thing you get when you first go into a room lighted by a low-powered mazda.

(B) Experiments with Naturally Lustrous Objects

Stimuli. From our collection of lustrous objects[3] *E* selected samples of different 'classes': textiles, shells, metals, and other minerals. To the stimuli used in the preliminary series we added a piece of lead, of the same size as the zinc, which had been rubbed with a fine-grained emery paper and then polished on a soft-cloth polishing wheel; a piece of red taffeta, a piece of blue silk, and a piece of red silk, all 4.5 cm square; a piece of royal purple satin, 60 x 49 cm; and a piece of flint having a lustrous area of about 3 x 5 cm.

Observers. The *O*s were H, F, G, K, W, P, Ha, and B.

Apparatus and Procedure. The *O* sat at a table in the dark-room, his head fixed by a Hering headrest. The stimulus, placed on the stage directly in front of him, was lighted by a 100-watt mazda bulb filtered through a Gage daylight roundel which stood at the left of the table, 55 cm to the left and 35 cm above the center of the stage. The stage was

[3]In addition to the lustrous objects which we brought together ourselves, we purchased from Ward's Natural Science Establishment of Rochester two collections of minerals: 'Collection Illustrating Kinds and Degrees of Luster: Physical Mineralogy 11' and 'Collection Illustrating Miscellaneous Characters Depending on Light: Physical Mineralogy 12.' We also purchased 50 polished shells, some from Ward's and the rest from W. F. Webb, Conchologist, Rochester, N .Y. The various items of our collection are mentioned in Section (E).

built as follows: we hinged one short side of a wooden frame, 30 x 20 cm, to a rectangular wooden base, 33 x 23 cm. Another frame, just small enough to fit inside the first, was hinged to one long side of the larger frame. A piece of grey cardboard, 19 x 29 cm, furnished with small pegs to hold the stimulus, was tacked to the inside frame. The stimulus could now be tilted toward the light and toward the *O* until it gave the best appearance of luster. H and B went through the stimuli and found only two positions necessary: for metals and shell the frame was set at 35° to the light and 35° to the *O*; for cloths and flint it was set at 12.5° to the light and 15° to the *O*. The only exception to this procedure was that the piece of royal purple satin was suspended from a horizontal arm directly under a skylight in a large light room and was observed from a distance of 183 cm.

The instructions for the group were now read to the *O*; no time limit was set for the observation.

The principal results were as follows: (1) luster is always independent of the surface of the lustrous object; (2) in texture it stands somewhere between film and bulk; (3) its quality is a peculiar commingling or sifting-together of dark and light.

(1) Luster as independent of surface

Most of our *O*s were consistent in placing the luster behind the surface, while others sometimes saw it above the object. They all agreed that luster is not a surface phenomenon; with the best luster, the surface of the stimulus-object is not seen.

(H, Black Taffeta, 1) Where it shows the best luster, the weave and surface of the cloth are hardly perceptible. If you turn to a part where the luster is not so good you find a surface, not quite a continuous surface, and the luster seems to lie behind this perforated surface. (H, Red Taffeta, 5) The luster is best in the areas where experience loses its perceptive character almost completely; it is not cloth, not even surface. (H, Zinc, 8) There is a clearly defined surface made up of very fine lines that are hardly perceived as lines. These lines are mostly light, although there are some streaks of dark. The luster lies beyond. (H, Shell, 19) In some parts there is definitely and positively a surface covering; the luster lies behind that surface and detached from it. In other parts, where the luster is best, there is no surface; there isn't much of anything there except the luster. (H, Red Silk, 20) There is a surface given by little streaks of dark or by little bits of brightness. Wherever there is a surface the luster lies behind it. (H, Zinc, 25) Behind the intermittent surface-effect, given by light and dark streaks or scratches, lies an indefinite mass; that is what I should call luster. (H, Black Taffeta, 26) There is a soft, glowy, lustrous mass behind the intermittent points of surface brightness. (H, Blue Silk, 27) The total of experience does not attach to the light surface-like network; there is definitely more behind this; vision does not stop off short at the surface.

(F, Red Taffeta, 16) The luster seems to be detached from the surface of the object.

(G, Purple Cloth, 49) A sort of thin cloud of very luminous light suddenly looms up, quite apart from the surface of the cloth. (G, Black

Taffeta, 40) The luster is a vapory experience, very hard to localize; a bulky experience of luminous greyish white. The surface itself is considerably more substantial and localized.

(K, Black Taffeta, 21) There are some parts which seem particularly lustrous; here there is a sort of bright mist, a mist which has nothing to do with the surface. (K, Lead, 28) In certain places a peculiar kind of brightness projects above the surface; it is soft and misty. (K, Lead, 30) There is a surface of dark patches and fine white lines; then there is the luster, a brightness which emanates from beneath and spreads all around like a thick, luminous fog.

(W, Shell, 32) There is a bright luster in some places and it seems to be back of the surface. (W, Red Taffeta, 44) It seems to fuse together; and then the shine as the shine of something disappears, leaving a luminous something. This luminosity is in front of the object, I think; a silverish grey. (W, Shell, 45) The brightness that is the shine does not seem to be associated with the object when it is just brightness; but when it is shine it is. (W, Blue Silk, 49) If I look at it as a piece of silk it all hangs together; but as color and light it is a part of a total experience which includes other things, particularly a detached glow. (W, Shell, 52) The brightness seems to belong to something that is behind the transparent smoothness.

(Ha, Red Taffeta, 21) There seems to be something behind the glistening area that is not glistening; just a shiny thing, a sort of soft glow. (Ha, Zinc, 39) There is something back of this shiny surface; a lot of infinitely small bright specks. (Ha, Black Taffeta, 19) There is something that seems to glow, to radiate light. At first it seemed to be on the surface; but when I went to say that I saw it, it was behind the surface of the object.

(P, Blue Silk,) That has a surface which is very smooth. It seems that there is a sort of brightness beyond that surface. (P, Black Taffeta, 7) It is smooth, bright, lively. There is a sparkly haze which is hard to localize; I can't say whether it is on the object or not. (P, Shell, 10) The lustrous part is very bright and very hard to localize. It doesn't seem to belong to the object.

(B, Lead, 9) There are a number of brilliant white parallel lines which seem to be quite apart from the luster which lies behind them. (B, Red Taffeta, 10) There is a layer of light which comes as a series of bright points in a plane, and back of that you have a dully glowing mass of light.

(2) The texture of luster

(H, Black Taffeta, 2) There is a great flood of light that is definitely bulky, massive, and that seems to lie beyond and shine through the surface. (H, Zinc, 3) In the lustrous parts the light is continuous and forms a bulky mass. (H, Red Taffeta, 5) The lustrous experience is a kind of bulky mass, although it is too loose for bulk and not loose enough for film; it is a sort of bulk-film, or filmy bulk. (H, Purple Cloth, 30) There is no definite surface given; it is bulky-like, it has depth, it is fairly soft and loose, but not enough to be a film. (H, Blue Silk, 31) You look into the experience, there is a realizable depth. It differs from film, which invites you to look into it but doesn't let you. (H, Black Taffeta, 26) There is a soft, glowy, lustrous mass behind the intermittent points that are localized; this mass is almost filmy in its soft looseness, in its lack of localization, in its apparent penetrability, in its

depth-like character, and in its lack of inherent limits. (H, Shell, 28) In some places the luster is almost pure; it is an indefinite mass that is somewhat more dense than film, although much looser than bulk.

(G, Black Taffeta, 30) The light parts seem to have depth; they are like whitish grey clouds. (G, Shell, 37) Some spots are very brilliant; they seem to lose their surface character and to become bulky. (G, Purple Cloth, 47) There is a fluctuation between a hard, sleek, brilliant, glassy surface and a soft, filmy, luminous cloud.

(K, Shell, 32) In the places where the points are very close together you get a nebulous, bright, cloud-like effect. A mist; soft, penetrable, filmy brightness. (K, Red Taffeta, 34) In looking at the thing casually the texture isn't evident and one sees only a uniform, luminous, penetrable, cloud-like lightness.

(W, Black Taffeta, 6) The shininess is like a silver film. (W, Shell, 11) If you had some sort of colored liquid with a diffuse light shining through it you might get the same effect. (W, Blue Silk, 35) The sheen seems to be 'sponge-work'; not a net-work because it isn't just surface. (W, Red Taffeta, 49) The difference between the shiny parts and the other parts is not just in tint or hue, nor in the distribution. There is something in the thing itself which is of a different material of experience; a little bit like frozen atmosphere.

(Ha, Red Silk, 21) The shiny areas are something which is not transparent; I don't know whether it extends back or comes forward. There certainly is not any one plane; it is as if the bottom had dropped out. (Ha, Lead, 25) The longer I look at it the softer it gets; less localized. (Ha, Red Silk, 30) In one very lustrous place there is just a deep pink light. All the other parts of it you could localize and actually touch. (Ha, Purple Cloth, 33) It has a subdued shiny appearance and looks diffuse and also soft. (Ha, Purple Cloth, 38) In the distinctly speckled part it looks as if there were a fine meshed curtain in front of the purple. And also the thing has more of a surface, whereas there is no surface in the white beaten up with purple.

(P, Blue Silk, 3) There seems to be a sort of brightness apart from the surface; a bright film. I can't quite limit the depth or thickness of this film. (P, Red Silk, 9) A sort of soft, bright, filmy stuff. (P, Red Taffeta, 12) I don't know any better way to describe it than to say it is filmy. (P, Red Taffeta, 13) The experience seems to be filmy, and the density of the film varies. (P, Lead, 16) There is a little sparkle about it; the sparkle is filmy or diffuse; not a clean-cut or pointed sparkle. (P, Purple Cloth, 30) It is characterized mainly by a grey filmy stuff; like a pool of grey filmy stuff.

(B, Lead, 5) Taking now what seems to stand as the luster itself, one has a mass of a peculiar kind of light; a glowy, self-luminous light that is too dense for a film but not so solid as a bulk.

(3) The quality of luster

(H, Red Silk, 5) There is a sort of bulk-film or filmy mass that is both light and dark, without either the light or the dark perceptible as such. It is not like grey, because there is both light and dark there. (H, Lead, 6) A soft, non-surfacy mass in which you see both light and dark without either being separable from the other. (H, Purple Cloth, 17) There is a semi-glowy brightness that is dominant. It is not bright points or bright anythings; it is just brightness; and all diffused through this brightness is also dark, a dark mass. (H, Zinc, 3) In certain re-

gions the light predominates over the black, so that light is continuous, and forms a bulky mass with the black somehow filtered all through it. No part of the mass of light is perceived as a point or as points of light. (H, Black Taffeta, 26) In appearance it is a mass of weak glowy light suffused with dark, or a dark mass suffused with a very weak glow. Neither the light nor the dark is perceptible except *en masse,* and the two masses seem to be in the same place. (H, Blue Silk, 27) The lustrous mass is mostly obscured by the surface; but where it does come out it is a soft, loose, indefinite, dark-bluish mass which is suffused with light. (H, Shell, 28) The luster comes as a soft, glowy mass of light that at the same time contains within itself a diffuse darkness.

(F, Purple Cloth, 21) There is an irregular spatial arrangement of light and dark areas. The arrangement is first of rather large patches and streaks; then, when these patches are scrutinized in turn, they appear to be non-uniform and are made up of light and dark.

(G, Black Taffeta, 1) There is a peculiar grey; it is like an impossible mixture of black and white; very, very fine granules of white and black mixed. (G, Zinc, 27) At first it is a sort of silvery granular grey with a glassy substance over the surface. As I look at it, this glassy substance tends to go away, and the grey breaks down into a very, very fine granular experience; a sort of very fine hash of grey points mixed with lighter points. (G, Black Taffeta, 40) There is a kind of powdery white which under close examination seems to be a mass of closely crowded, very, very minute white points in a black medium. (G, Red Taffeta, 43) The rose color seems to deepen and thicken and become whitish; a sort of intense, fine, granular mixture of dark rose[4] and white. (G, Purple Cloth, 47) This cloud is not of uniform quality; there is an extremely fine mixture of rather dark purplish spots and brilliant white spots. (G, Purple Cloth, 48) Sometimes the glass disappears, and the experience becomes a powdery mixture of white and dark purple; the two seem somehow fused (not mixed) into a single quality.

(K, Black Taffeta, 3) Soft and velvety dark with white spots; these spots are very minute; tiny grains of dust. (K, Purple Cloth, 40) This brightness is not homogeneous, but is very minutely differentiated into light and dark bits; these bits are not sharply cut off from one another, but seem to merge gradually into one another. (K, Zinc, 47) For description it is very difficult; you just know somehow that there are both dark and light there.

(W, Purple Cloth, 58) It is a mist of brightness and darkness, but the whole seems to mean brightness. (W. Zinc, 58) There is a mist, just at first bluish, then colorless. Casually it is all light; but critically it has both light and dark in it somehow.

(Ha, Purple Cloth, 35) There are tiny, tiny pinpoints of white, but these are only discernible with careful scrutiny; it is just purple beaten up with white. (Ha, Purple Cloth, 37) In the most lustrous parts these pin-points fuse together, and it becomes just white beaten up with purple. (Ha, Purple Cloth, 38) It is just white all beaten up with a darker purple; it sometimes seems to be nothing more than very fine white chalkdust scattered through purple. (Ha, Zinc, 39) There is something back of this sheeny surface; it is light all beaten up with bluish-grey. (Ha, Shell, 40) There is a dark grey beaten up with a silvery brightness.

[4]Some of our *O*s identify the body-color of the object with the dark component of the luster. For discussion see p. 152.

(P, Lead, 18) An irregular mixture of brightness, but a mixture that is so fine that there is no real separation. (P, Purple Cloth, 20) Greyish-blue stuff, all beaten up together; no points or separation between the light and the dark.

(B, Lead, 25) A kind of light somehow mixed up with a darker quality; the light is not separate from the dark. It is as if light and dark qualities were just beginning to separate.

In the reports of all *O*s regarding the quality of luster we can trace—not always with the same clearness, to be sure, and not always with a well-marked differentiation of stages—a certain uniform trend. The *O*s begin, all alike, by speaking of 'specks,' 'flecks,' 'spots,' 'points,' 'streaks' of light, which either appear against a dark background or float in a dark medium or alternate with dark flecks or spots. Later on, they speak typically of a 'dust' or 'haze' or 'very fine hash' of light and dark, or of a 'powdery' or 'minute granular mixture.' These terms, in their turn, prove unsatisfactory, and give way to 'interpenetration,' 'commingling', 'sifting through,' 'fusion that is not mixture,' 'sprinkling without separation,' 'light and dark, somehow seen as if in the same place at the same time.' When once the *O*s have ventured upon phrases of this sort and have braved the apparent paradox that they involve, the reports do not change.

To explain this trend, we must bear in mind that the stimuli (perhaps the instructions also) of Group (A) favored the object-consciousness. As the experiments progressed and the interest of the *O*s shifted more and more from object-surface to underlying luster, that consciousness weakened; but we are very doubtful if it ever—except for H and B—completely disappeared. At the beginning, it was present in full force; and the luster was accordingly perceptualised; it was regarded, so to say, as a softer 'surface' lying behind the surface proper of the object, the hard surface. And as the surface proper was constituted of flecks or spots or patches of light and dark, so the perceptualised lustrous 'surface' was characterized, by analogy, in those terms. In the early experiments, therefore, the *O*s are reporting perception rather than phenomenon; they are trying to get a grip upon the phenomenon of luster—a very slippery and labile thing—by means of perceptive analogy. After a while, with continued observation, the analogy breaks down; but the object-consciousness still insists that a field in which both light and dark are seen at once must show these characters in spatial separation, must be a granular or particulate field. With observation still further continued, phenomenology comes to its rights, and the reports take on the para-

doxical form (one of the *O*s spoke of his statements as 'absurd') that we have noted as their final stage.

The explanation outlined above is based upon accounts given independently by B and H, and upon incidental remarks made by other *O*s. B says: "When I began, I was utterly at a loss for words; my attention was on the luster, and my interest was in the luster, but I was completely baffled in trying to describe it. I fell back on words that meant 'speckled;' all my collection of stimulus-objects had 'speckled' surfaces, and I tried to carry over this surface-speckling to the luster. I did not know what luster was really like, and the analogy gave me a start; I think, all the same, that I knew I was not really describing luster; anyhow, almost as soon as I had started, I tried to find better terms. Even after I was clear about the phenomenon, though, I hesitated to say what I really saw; it seemed self-contradictory. It was curious at the end to see how far away I had got from surface; if I try now to get a perception of two speckled surfaces, or anything like that, I can't do it."

H says, very similarly: "For me at first it was a sheer difficulty of description. If it had not been for old language-habits, I think I should have seen what I saw later; as it was, the difficulty of description (combined, I daresay, with other less obvious influences) forced me into a perceptive attitude. In everyday life, if what we see is made up of light and dark it must be made up of light and dark points or areas. The experience was thus forced into ordinary perceptive terms, and it took me some time to come to a realisation of non-perceptive light and dark; I saw the lustrous field under a sort of memory-color. I remember that it came with a strong flood of pleasure when I hit upon the final descriptive formulation and could be sure I saw things as they were. I seemed to have been on the point of seeing that same phenomenon all along, but now I have found the way to say what I saw; the perceptive attitude broke down for good as soon as I freed myself of the terms of perceptive characterisation."

Since the above results hold for all the stimulus-objects used, we may infer that luster is everywhere the same; it does not vary with the nature of the object. We planned the experiments of the fifth Group as an experimental test of the validity of this inference.

(C) Experiments with Synthetic Luster

We set out now to produce a luster quite apart from any object or surface, to synthetise a 'reduced' luster. Of several procedures tried, two were successful; the transmission of light through textiles, and the use of luminous paint.[5]

With both modes of stimulation H and B reported the experience to be that of luster, and gave descriptions which were identical with those of Group (B). The other *O*s, who were not familiar with the object of the investigation, never called the synthetized experience 'luster;' but their accounts of it also tally perfectly with

[5]Devoe's Luminous Paint.

those of the previous series. In everyday life, the meaning of luster, is, of course, bound up with the object-consciousness. We shall have to discuss this point more fully at the end of this Section.

(1) Experiments with transmitted light

Under certain conditions, a light shining through finely woven textiles, sets up reduced luster. We had one sample of very closely woven satin which we could use in single thickness. Ordinarily, however, we found it necessary to double the materials upon themselves across the grain; otherwise a few points of high light would stand out distinct from the lustrous field. H and B found that these bright points were localized in a plane in front of the luster and made a surface.

Apparatus and Procedure. Into one side of a light-box, 27 x 16 x 19.5 cm, we inserted the larger end of a cardboard truncated cone 33 cm long with a diam. of 8 cm at the smaller end. The box was placed on a stand, 15 cm high, with the cone standing upright. The stimuli—black taffeta with red silk, two pieces of blue silk, blue silk with red silk, and the satin above referred to as exceptionally closely woven—were laid across the top of the cone. A mirror, 40 x 30 cm, was hung vertically on two standards in such a way that it could be revolved on a horizontal axis, and was placed 17 cm behind the light-box with the center of the mirror 43 cm above the top of the cone. A rheostat controlled the intensity of the light, which was given by a 100-watt mazda bulb. In front of the set-up we placed a grey cardboard screen, 70 x 110 cm, having a rectangular opening 14 x 28 cm through which the reflection of the stimulus appeared. *O* sat 1 m in front of the screen.

H and B went through the stimuli to determine the most favorable intensity of light and angle of mirror; they also assured themselves that the use of the mirror made no difference in the look of the stimulus. Before every observation *E* laid the cloths to be used across the top of the cone and looked at them from *O*'s position to make sure that there were no bright points.

The instructions were read to the *O*; no time limit was set for observation.

Observers. The *O*s were H, G, K, W and B.

(2) Experiments with luminous paint

One ordinarily thinks of luminous paint as 'glowing.' We had, in fact, planned to use it, in place of the light-box, as a source of illumination. It turned out, however, to be not so bulky as true 'glow,' and to give a perfect 'reduced' luster. Not only is it free from object-reference and indefinite in localization, but also—after the first few moments of scrutiny—it loses color quality and appears simply as light-dark.

Apparatus and Procedure. We used the same experimental arrangement as in the experiments with transmitted light, placing the stimulus,

a roughly rectangular patch of the paint, 6 x 4 cm, spread rather thickly on a piece of cardboard, upon the cone. Before an observation hour the stimulus was exposed for 10 min. to the light of a 100-watt mazda bulb.

It was necessary in these observations to allow a very brief period (30 sec. sufficed) of dark adaptation. The instructions were read to the *O*; no time limit was set for observation.

Observers. The *O*s were H, F, G, K, Ha, W, P, and B.

Besides showing that luster may appear independently of objects and their surfaces, the results of these experiments confirm those of Group (B) as to the texture and quality of luster.

(3) The texture of 'reduced' luster

(H, Black with Red, 1) The whole thing seems to be massive; there is no surface. It is really a bulk, a filmy-like bulk. (H, 2 Blue Silks, 6) Yes, it is a good luster, but not the best luster I have seen. The mass is a little bit too near to bulk, not quite filmy enough to be the best luster. There is nothing like surface. (H, Blue with Red, 7) Very good luster. The whole is indefinite in localization and is rather soft, loose, not quite pure film nor yet dense enough to be bulky. It seems to have depth, like a film; but the depth is a little more certain than that of a film; you really can see into it, it is penetrable. (H, LP, 8) There appears in the midst of a rather dense black, that is somehow contoured, a mass of light-dark experience that has depth, that is something like film and something like bulk in its nature.

(K, 2 Blue Silks, 5) The whole thing is shadow-like in appearance; a loose, filmy, mist-like experience. (K, Blue with Red, 8) There is a patch of light and dark out there in front of me which is shadow-like, soft, fuzzy, rather loose in texture, cloud-like in nature; somewhat bulky. (K, 2 Blues, 9) The experience is bulky, but it is a soft, cloud-like kind of bulk. (K, LP, 10) The patch as a whole is not flat but somewhat bulky. (K, LP, 11) The experience does not come as a surface, but rather as soft and fleecy like a dense cloud. (K, LP, 13) An oblong patch which is cloud-like, fleecy, soft, penetrable, tridimensional. (K, LP, 14) A patch with indefinite boundaries which is like a fleecy, nebulous cloud; like a filmy-bulk. (K, LP, 18) It is a soft, cloud-like blending of light and dark, dense yet penetrable.

(G, Blue with Red, 2) There is a considerable depth here, almost as much as in a bulky color. (G, 2 Blues, 6) A very soft, loose experience that has a great deal of depth, like a bulky color. (G, LP, 8) The experience has a very soft, almost filmy texture. (G, LP, 9) It is a very thin, loose vapory, misty experience. It is like a film color. (G, LP, 11) When I first opened my eyes I saw a mass that was like a bulky color; it later became filmy and looked like a cloud of luminous dust. It has depth; at the back it is more dense, while toward the front it is more loose and vapory. (G, LP, 12) It is a bulky experience like a dense, luminous gas without any definite contours.

(W, 2 Blues, 2) I guess it is tridimensional; it is not like a thing. It is fluffy. It is somewhat like an after-image. (W, Blue with Red, 4) It is a brilliant film; there is no surface, but it all seems a part of the soft, indefinite mass. (W, LP, 10) This is a filmy floating patch; a vaporous kind of thing, very soft in appearance. (W, LP, 11) A filmy, cloudy, loose brightness. It seems deep, but not exactly tridimensional. (W, LP, 13) It is something like transparent, but not quite that. You

can see into it but not through it; it is just a haze of a peculiar kind of brightness. (W, LP, 15) A filmy experience, not a thin film but rather deep. This filminess seems part of the brightness of the experience.

(Ha, LP, 3) It looks airy, like frozen air. It is soft, as if it could be pierced. (Ha, LP, 4) It is diffuse, smooth, even, has no surface. It is a vapory thing. It goes on indefinitely beyond itself. It looks like a cloud or fog. (Ha, LP, 7) It is soft. It is a film, almost. (Ha, LP, 8) It is a diffuse, thick film.

(P, LP, 1) It is more or less filmy; soft, loose, and penetrable. (P, LP, 7) It is an almost filmy experience. (P, LP, 8) It is like a dense film; there is certainly something filmy about it.

(B, Black with Red, 2) The experience comes as a mass of light-dark that is like bulk in its density but has many of the characteristics of a film; it is loose, invites you to look into it, seems to suggest rather than show depth. (B, 2 Blues, 3) There is that same kind of filmy-bulk that seems to be characteristic of luster. (B, LP, 6) There is no surface; it is very much like a pure film, but a little like bulk in its density. The depth is a bit more definite than the depth of a film.

(4) The quality of 'reduced' luster

(H, Black and Red, 1) The area is almost uniformly lustrous. It is light and dark; yet I simply cannot pick out points of light or points of dark. That seems to be what luster is—light and dark, neither of which can be individualized. (H, 2 Blues, 6) There is darkness, with light sifting or filtering through it. It is an inter-penetration of two filmy masses, one light and the other dark. (H, Blue with Red, 9) There is a glowy mass that doesn't break up into discrete parts; and yet there is, in the same place, a soft mass of dark that also doesn't break up; the two are simply sifted through one another. (H, LP, 8) There is a mass of glowy light; and powdered or sifted all through this mass of light is also dark; it is not dark bits but just dark. The light and the dark seem to be all beaten up together.

(K, 2 Blues, 9) The experience is of darker and lighter patches. These patches somehow seem to go all together, to run into one another. (K, Blue and Red, 8) There are dark and light patches, cloud-like and soft. They seem to form a glowy mist. The light and dark patches seem to merge into one another. (K, LP, 12) There is a continuous differentiation into lighter and darker quality; but there are no distinct boundaries between the light and the dark, they just shade into one another. Even 'shade' is not a good term; it implies a gradual blending off. What you really have is just that over the whole area there is this differentiation although one cannot, in any given spot, pick out bright or dark bits. Just a general differentiation into light and dark. (K, LP, 15) There is light and dark, but not as discrete things. In general, there is an area in which there is a differentiation into bright small extents, and dark small extents,—except, of course, that you cannot see them as extents. It is just a vague differentiation into dark and light. (K, LP, 16) The cloud is of light and dark, but there is no distribution of parts. It is a sort of blending or fusion of light and dark.

(G, 2 Blues, 1) A sort of granular mixture of blue and tiny white points. These tiny white points tend to fuse together. (G, Blue with Red, 5) It is a sort of powdery mixture of white with a darker, rose quality. (G, LP, 13) There seem to be two experiences, a dark grey

and a white; sometimes they seem to fluctuate, at others they seem to fuse, occupying the same space.

(W, 2 Blues, 4) Once in a while there is a brilliant film; just lightness, that hasn't the character of a thing at all, and it seems to form a part of the soft, indefinite dark. (W, LP, 10) It almost seems to resolve into little particles—little particles of light with darker quality between them. (W, LP, 14) It is a haze of very finely divided particles of light and dark. (W, LP, 15) The whole thing acts as if it were on the point of dissociation in some way. There seem to be some differences of shade that make me call it 'particles'; it isn't quite homogeneous. (W, LP, 18) The whole thing is a light, everchanging mist; it is a mist of light and dark. The light and the dark do not seem distinctly separate; it is as if the dark were always there with the light shifting around in it. (W, LP, 20) There is a mist of lightness and darkness. A part is now dark and now light; only, the word part is too big—the bits are too fine to be really seen as definite parts. (W, LP, 22) It is an insubstantial mist; light is suspended in dark; somehow enveloped in dark.

(Ha, LP, 12) It reminds me of a light coming through a fog of darkness. (Ha, LP, 21) It is like a white beaten up with a dark purple; there are no separate points.

(B, Black with Red, 1) The characteristic of the experience is a peculiar admixture of dark and light in which the dark and the light seem to be all beaten up together. (B, 2 Blues, 4) Somehow, quite apart from the blueness, there is a fog that seems to be made up of light and dark; you cannot pick the two apart. (B, Single Satin, 6) This gives a very good luster. There is a kind of bulky, smooth mass that seems to be made up of light and dark; it isn't simply light, but there is a quality of darkness there too which is quite as observable as the light.

(5) Comparison of 'object' luster with 'reduced' luster

We have said above that no *O*, except H and B, used the word luster to characterize the 'reduced' phenomenon, though all had used it, in the experiments of Group (B), to characterize an apparently identical phenomenon. We thought that the failure to employ the term was probably due to the absence of a localized surface; and since, in determining the conditions for 'reduced' luster, H and B had found that bright points showing in the field might form a surface in front of the luster, we tried to bring the other *O*s to a use of the word by adding a number of such points to the stimulus. With the first procedure, using transmitted light, we introduced bright points by increasing the intensity of illumination: in the second, using luminous paint, by pricking fine holes in the stimulus and letting a very faint light show through from beneath.

For H, these points formed a surface, as before. But K, W, and Ha reported that they either (1) stood out in front, or (2) were embedded in the luster.

(H, Black and Red, 1) In front of the luster there are a lot of tiny

bright points which are all in a plane and form a net-work surface. (H, 2 Blues, 2) There are a lot of specific points of light which stand on a plane. They seem to be the things I hitch to perceptively. (H, LP, 3) There are a great many little bright points, again, which stand in a plane. The luster lies beyond them and yet comes right up to them. They give the perception of something like a surface.

(K, 2 Blues, 1) There are definite bright points which are embedded in the stuff itself. Sometimes they look like hollows. (K, Blue with Red, 2) Scattered all through the experience are bright points that are definite points and seem to lie at different depths. (K, LP, 3) It is a blending or fusion of light and dark, with a lot of bright points in 'disorderly' arrangement somehow embedded in the mass.

(W, 2 Blues, 1) A glowy, luminous circle dotted with brilliant lights. (W, Blue with Red, 2) There are hard, glittering, bright spots in a glowy background. They give a sheen to the experience. (W, LP, 3) There is a patch of luminous blue with a pattern of reddish lights in front of it. (W, LP, 4) There are lights that almost twinkle. These are not really out in front of the blue but in it. It is like a haze studded with bright points.

(Ha, 2 Blues, 1) There are little sparkly pin-points. They seem to stand out in front of the rest. (Ha, Blue with Red, 3) It looks like a lot of little stars in a pink sky. (Ha, LP, 3) It is just an airy, purplish area with a lot of bright points in front of it.

We took great pains to vary the number and size of the holes, tried haphazard and orderly arrangements, and used different degrees of brightness; but we still failed to get a report of luster from these *O*s.

We next called for a series of observations with two stimuli only, the luminous paint in the dark room, and the large piece of purple satin in daylight. The reports are practically identical. We subjoin pairs of reports, the one member referring to the paint, the other to the satin. It is impossible, without further cues (color, size, etc.), to assign them to their proper stimuli.

(G) A thin cloud of luminous light suddenly looms up.

It is a soft filmy thing like a cloud, which looms up suddenly and glows.

(K) It is a soft, cloud-like blending of light and dark.

It is glowing, soft, filmy; not homogenous, though the light and dark merge gradually into each other.

(W) It is a mist of brightness and darkness.

It is a mist of light and dark.

(Ha) It has a diffuse, soft appearance.

It is soft, filmy, diffuse; looks like a cloud or fog.

(P) A pool of filmy stuff.

It is like a dense film.

(F) It is a suffusion, an interpenetration, of light and dark; the light and dark are both present as light and dark but cannot be separated.

It is a mist in which dark and light are both there but cannot be separated or separately localized.

In this series, too, the satin was termed lustrous and the luminous paint was not. It was therefore necessary to confront the

*O*s with their reports, and to ask them why they made this distinction between the stimuli.

(G) The satin is an object, for one thing, and I always think of luster as being on an object. And then at times the satin became a thing with a surface, a shiny bit of cloth. I don't know how else to account for it. If there is such a thing as abstracting luster from objects—if you could get the luster away from the cloth and call it luster—then I should call the luminous paint lustrous.

(K) There seemed to be a sheen to the cloth when I looked at it just as a cloth; but when I came to describe it, the two were alike. They were alike in that the differentiation into light and dark, which gave the glow-like effect, was the same thing. But I could look at the satin as a whole, and get a surfacy flatness. I think of luster as the luster of a surface.

(W) Yes, but the cloth is an object, I never thought of luster except on an object.

(Ha) The cloth was an object and it seemed to shine. What I saw in the dark room was the cloth without 'clothness'.

(P) I think the general situation is what makes the difference. The luminous paint is lustrous, but one thinks of luster popularly as attaching to an object; and so the cloth would more naturally be called lustrous.

(F) Luster for me is a term that I attach to a familiar experience that I have always had in connection with some object. So luster always meant the lustrous surface of an object.

As a matter of fact, the satin, at the distance at which it was viewed, gave to a critical observation (H, B) no trace of surface.[6] The decisive point was, then, that the satin was a perceptive object, the patch of paint a phenomenon: given a stimulus-object, and it is called lustrous; given a phenomenological datum that is like it in all respects save objectivity, and (in default of special training) the adjective is not available.

(D) The Lability of the Lustrous Field

We have remarked above that luster is (to use Katz' word) a labile phenomenon.[7] If it were not, the 'paradoxical' reports mentioned above would not have been given. Luster, as a bulky film, forbids fixation. In the experiments of Group (B), the *O*s could always bring their eyes to rest by fixation of some point of the object-surface; in those of Group (C) there was no chance for fixation, not even a clean-cut contour of the lustrous field. We therefore expected that reports of inner movement or instability would be frequent in the experiments of Group (C), much less frequent

[6]The shiny surface seen by certain *O*s was, undoubtedly, an imaginal overlay: see G. K. Adams, An Experimental Study of Memory Color and Related Phenomena, *Amer. J. Psychol.*, 34, 1923, 359 ff.

[7]D. Katz, Die Erscheinungsweisen der Farben, *Zts. f. Psychol., Ergänzungsband* 7, 1911, 24.

in those of (B). We also expected that the experiments of Group (C) would bring complaints of eye-strain.

In fact, however, our *O*s do not say much under this heading. In Group (B), where we had 8 *O*s and 504 reports, we have mention of the instability of the phenomenon from 4 *O*s in 14 reports. In Group (C), with the same 8 *O*s and 480 reports, we have mention of instability from 6 *O*s in 21 reports. B alone refers to eye-strain, and he only once.

Group (B): Objects

(G, Red Silk, 34) It is continually changing, moving as if it were alive. (G, Lead, 36) The experience is very instable and it is continually shifting in tint.

(K, Red Silk, 29) Sometimes dark is predominant and at other times light is predominant. The change is like the shift in perspective in the reversible illusions. You can see it as either one thing or the other; either as bright or as dark.

(W, Black Taffeta, 9) There is something almost like movement in the experience. (W, Red Silk, 10) There isn't exactly a movement in the experience and yet there is some sort of expansion which I cannot catch. (W, Blue Silk, 18) The bright and dark seem to move, to change places from moment to moment. (W, Red Taffeta, 25) There is something which almost seems to move and yet nothing really does. (W, Shell, 52) The luster isn't static and still it really doesn't move. It is as if perhaps it was going to move. (W, Purple Cloth, 56) This experience is static but somehow gives the impression of change. (W, Purple Cloth, 57) The whole appears lustrous, fluctuating, almost moving.

(P, Red Taffeta, 17) There is a great deal of fluctuation, of variation of some kind. (P, Purple Cloth, 24) There is some kind of a change going on.

Group (C): Synthetics

(B, Blue with Red, 1) There seems to be a constant change of some kind; I think it is in focus. I get severe strains around the eyes.

(K, Blue with Red, 8) The dark seems to fluctuate: sometimes it is prominent and at others it recedes. (K, LP, 9) This luminous cloud is homogenous except that it is filled with dark points. Well, sometimes these points are light; I am not sure always. It is hard to say because they are so numerous and shifting. (K, LP, 10) In general, it is an area in which there is a differentiation into bright and dark infinitesimal bits which are continually shifting. (K, LP, 13) There is a continual shifting and changing of the light and dark parts.

(G, Blues, 1) A granular mixture which seems to be shifting and moving. (G, Blues, 5) Although the experience is weak, not intense, there is an insistence about it. There is a liveness within it which is almost an effect of movement. (G, LP, 10) It is more intense than its whiteness would seem to warrant. There is a lack of connection between the whiteness and the intensity. Perhaps liveness is a better word than intensity. (G, LP, 16) A ghostly sort of experience which seems to be floating around in the air. It is a lively thing; the inside seems like a squirming mass, changing all the time. There is a meaning of move-

ment, but I think it is only a shift from grey to white and white to grey again.

(W, Blues, 2) There seems to be movement of tiny bits. (W, Blues, 4) The whole thing is glowing. It is still, but for some reason it suggests movement. (W, LP, 9) It isn't like a thing; it is like a lighter, flaky, moving part of the dark. (W, LP, 10) It is a sort of a silvery, moving, floating patch. It is continually shifting. This shifting may be a shift of brightness. It is dark and then the dark moves and it becomes all light. But even when it is all light there is some kind of a change or difference. (W, LP, 11) It is a buoyant thing, there are changes, in brightness, I think. At any rate there is a sort of movingness. (W, LP, 12) There is a kind of moving of light in a darker background. The whole gives the impression of movement, or rather of a tendency to move. (W, LP, 13) Tiny points of brightness are blended into one whole. It is an instable, constantly shifting, interplay of brightness. The whole thing it static in its surroundings, but the impression is one of movement. (W, LP, 14) It is a haze of very finely divided particles of light and dark. They move constantly as if vibrating. (W, LP, 15) It is a mist of light and dark in which the light and the dark do not seem to be distinctly separate. It is as if the dark were always there with the light shifting around in it.

(Ha, LP, 12) It seems quite 'alive' today. It has stability, and yet it seems to be fluctuating within the stuff.

(P, LP, 11) There is a continuous change in brightness.

These reports are, fortunately, enough for illustration. We discovered, after the experiments were over, that those *O*s who were familiar with the phenomenology of bulks and films had taken the instability of luster as a matter of course, and had concentrated accordingly upon a description of the twofold quality, which was a novel feature. The other *O*s, in some cases at least, looked upon the instability of the field as an annoying character of the stimulus, and so did not connect it with the phenomenology of luster. *E* noted that every *O* in the experiments of Group (C) frequently showed outward signs of eye-strain.

A difference in our *O*s' reports raises the question of the relation of luster to the body-color of the lustrous object. Some *O*s never mention the color of the object in their descriptions; others make it the dark component of the luster in the case of darker objects, particularly the purple cloth. Three possibilities suggest themselves: (1) with dark colored objects the color may become the dark component of the luster; (2) with light colored objects the color may become the light component; or (3), as is certainly true in many cases, the luster may appear entirely apart from the color of the object. *E* is inclined to believe, on the basis of a few special observations, that the last of these statements represents the facts. He finds that, while it is possible, by attitude, to bring about (1) or (2), yet if he takes the lustrous experience sheerly

per se, it is always a fusion of light and dark independent of the color of the object.

We plan to undertake systematic experiments on this question.

(E) Classification

We have said in the Introduction that luster is always the same, and that the different 'kinds' of luster are in fact different modes of the surface-appearance of lustrous objects. The first suggestion to this effect came from the result of Group (B); the uniformity of the descriptions led us to infer that luster does not vary from object to object. To test the inference, we asked H to classify 114 objects ordinarily accepted as lustrous and to give a phenomenological account of every class.

Procedure. We laid out the 114 objects on a table in diffuse daylight. The instructions were as follows: "I want you to classify these objects according to their general appearance. To assist you I am giving a list of terms frequently used to characterize objects of this general type.[8] You need not confine yourself to this list, but may add any new terms or leave out any given here as you see fit."

Metallic	Metallic-pearly	Sub-vitreous	Glimmering
Sub-metallic	Silky	Resinous	Graphite
Adamantine	Splendent	Greasy	Satin
Metallic-adamantine	Shining	Pearly	Mother-of-pearl
Vitreous	Glistening		

O was allowed to pick the objects up and examine them at any angle. H classified the objects into 31 groups. His list omitted 4 of the terms given in the instructions and added new categories which were either combinations of two terms ('Satin-silky,' 'Metallic-silky') or original terms qualified ('Soft silky,' 'Hard pearly'). After the task was completed H gave a statement of his procedure: "I took the terms given to me and tried to find things that would fit those categories. I very soon found that, for example, not all silks look alike; and so the compound terms came in simply because there were objects which did not seem to belong to any of the original headings. The whole thing was sheerly a matter of common-sense impression."

After H had finished making the classification we asked him to give us a phenomenological description of every class.[9]

(1) *Silky* (brocaded satin, brocaded grosgrain, satin messaline, chiffon taffeta, chiffon cloth, crêpe de chine, china silk, chiffon crêpe, moire taffeta). There is a fine network of bright points that give a bright, almost shiny, surface. Behind this is the lus-

[8] To Ward's list of 'Kinds and Degrees of Luster,' given in the 'Collection Illustrating Luster: Physical Mineralogy 11,' we added a few other terms which we had found used in earlier articles on luster. We reproduce the list in the order given by Ward.

[9] We give the following reports in the order in which they were made by H.

ter; a filmy-like texture with a strong suggestion of depth. It is a very light glow containing within itself a quality of darkness. This light-dark film is interrupted by the points of the surface.

(2) *Soft silky* (lingette, 2 pieces). In the soft-silky the light points are a bit less definitely formed and the network surface is less definitely localized. There is the same soft, glowy mass behind the surface. It is a light-dark, glowy, film-like appearance.

(3) *Velvet-silky* (baronette satin, chiffon velvet, crêpe de chine-2).[10] In these objects there are very small bright points that are relatively widely separated, and dark, hard dashes that occur at irregular intervals over parts of the object. Together these form a bounding plane. Between these points and dashes is a soft, glowy mass, without definite surface, that is lustrous. It is a light, filmy glow interpenetrated with darkness.

(4) *Satin-silky* (changeable taffeta, satin messaline-2, taffeta silk, black taffeta, cotton-backed satin). With satiny silk the bright points are a little larger and more definite than in satin, but less than in silk. The total effect is that of something a bit harder and a bit more reflecting than satin, but not so much so as silk. The luster is the same sort of glowy mass behind the surface.

(5) *Metallic-silky* (lead-foil, satin-finished brass). This class presents extended areas of shiny surface like the metallic objects. It has the sheen of new silver. The surface is broken into much more definite points of light and dark than is the case with the metals. Also the surface is more definite. The whole thing is harder, more dense and compact. The light and dark points break up the surface; it does not give reflection. The denser shiny surface-effect is much like silk. In certain parts a mass of glowy bright and dark lies behind the surface and here the surface is less definite. It is this darkness and glow, all beaten up together so that both occupy the same space, that is the luster.

(6) *Shiny-silky* (tinsel fabric, tinsel moire-2). Not lustrous like the others. There is a hard, shiny surface of bright streaks and points interspersed with dark streaks and points. The darks do not belong to the surface and only incidentally belong to the object. The shiny points and streaks give a surface sheen, particularly as they are enhanced by the intervening darks that do not belong to them.

(7) *Pearly* (Unio plicatus, Astralium cooki, Turbo argyrostoma, Colliostoma conulum, Haliotis cracherodi, Delphinula lacinata, Harpa ventricosa, black lusterware). In the pearly ob-

[10]When a stimulus is followed by '-2' or '-3' the figure indicates a second or third stimulus of the same kind.

jects there are sometimes surface reflections of light, but for the most part the reflections are from below the surface; bulky reflections that are usually colored. Between these spots are lustrous areas or dead, dull surface-areas with the lustrous areas predominating. The luster is a soft, filmy-like area that lacks surface, has the appearance of depth, and appears as a glowy dark-light. In some objects it is predominantly dark and in others predominantly light.

(8) *Metallic-pearly* (Turbo marmoratus, Patella toreuma, Blue Haliotis, Haliotis stomatiforma, Turbo suffusus). In the brighter parts of the objects there is no luster: there are only surface and bulk reflections. In some of the less brilliant parts there is some luster, and in these lustrous spots the object presents no surface—just a filmy-like loose appearance that suggests depth or mass. This apparent mass is both light and dark at the same time.

(9) *Hard pearly* (Unio verrucosus, Unio gibbosus, Unio ligamentinus, Astralium undosum, Gibbula albida, Unio tuberculatus, Modiola plicatula, Astralium triumphans, Turbo sormaticus, Oliva litterata, Heulandite). The general impression is of bulk and hard glinty surface. It is almost all bulky reflections, with some surface reflection here and there. The whole experience is hard, dense, glassy. For any definite position of the object there are parts of the object where the glassy surface is very thin or is entirely absent; and here we get a definite luster. The luster is the same sort of thing I have described before.

(10) *Satin-pearly* (Unio alatus; Mytiles, New Zealand; flint). These objects have the glassy-like character of shells and yet there are no reflections and no definite surface marking. There is a softness or diffuseness, due to the lack of a definite surface, and this softness gives the satin-like appearance. There is luster.

(11) *Splendent-pearly* (Chlorastoma sp., Haliotis gigantea, Haliotis cracherodi, precious opal). Here again we have a definite surface. The high lights are smaller bits with somewhat less definite limits. These high lights are colored, which gives a hint of glow to them as well as a certain depth. Mixed with these colored lights there are others that are sheer brightness, and it is this mixture of various colors and brightnesses with the glowing character of the colored ones that gives the splendent aspect. As in *hard pearly,* the luster lies outside the lights; in appearance a kind of subdued glow, a glowy light with dark all beaten up with it.

(12) *Greasy-metallic-pearly* (Trochus miloticus, Haliotis sp., Oliva porphyria, Clanculus puniceus). Scattered here and there

among the bulky color reflections that are like the *pearly,* and the surface high lights that are like the *metallic,* and a few dull areas, there are a few patches of luster. These different areas all seem to run into one another, and that seems to be what gives the greasy appearance. The glow of the luster seems almost to run into the dense bulky reflections; and yet there is a difference. This difference is that the glow of the luster has somehow a darkness in it.

(13) *Metallic* (lead, mica, copper, galena, silver). The characteristic feature here is large, smooth, almost glass-like reflecting surfaces. There is no translucency; but the reflecting medium gives a general depth-impression, just as objects appear to be behind any reflecting surface. There is high brilliancy as the surface is turned to the light. The objects may show color or even a variety of colors. Always there are hard, smooth, extended reflecting surfaces. The angle that the object has to the source of illumination must be controlled very carefully to get luster at all. The bits of luster that appear are soft masses lying behind the surface markings; they appear as luminous light interpenetrated by darkness.

(14) *Satiny* (satin duchesse, satin messaline-3, satin charmeuse, satinwood). *Satiny* differs from *silky* mainly in the fact that the bright points are smaller and less definite; they form, with the dark interspaces, the proximate surface of the lustrous mass; in *silky* the mass is behind and detached from the surface. Satin comes nearer to pure luster than does silk. At their lustrous best the satiny things have no definite surface. There are surface-cues all about the limited areas of best luster, so that they are out there at about a certain place in space; but there is no definite starting-place. They also have the appearance of depth without there being any definitely given amount of depth. The luster appears as a fairly loose film-like mass that is predominantly light permeated with darkness.

(15) *Silky-satiny* (changeable messaline). This object possesses the light and dark of the surface of silk, only here the transitions from light to dark are not abrupt, so that the whole has a soft appearance like satin. Through the discontinuous surface constituted by the weave is a glowing soft light interpenetrated with dark.

(16) *Metallic-satiny* (silvered paper, old gilt, old copper, gilded paper). At first glance these objects present the extensive uniform surface of the metallic objects. They have the shine of silver and gold. But it is not a reflecting, smooth surface. They give something like a dull glow rather than a brilliant high light. The surface is slightly indefinite; it is alternately light and dark and rather soft. Behind the surface is an indefinite mass of light and dark.

This, with the softness and the indefinite surface, is like satin. This light-dark mass is the same thing that you always see in a lustrous object.

(17) *Graphitic* (Graphite). Graphite presents poorly defined high lights; poorly defined in contour. It is characteristic of this object that the light parts are, except for the very lightest, interspersed with dark. In some parts the light and dark are broken up so finely that the separate parts become non-perceptive. There are just light and dark diffused through one another, and these areas are typically lustrous. On these parts there is no surface, but on the larger lighter parts there is a definite surface.

(18) *Metallic-graphitic* (Board painted with 1 coat of black lead and white shellac, stovepipe satin). Bright points or short streaks that have the shine of metal and that lie so close together as to give a very definite surface. This surface, taken casually, is like the surface of metal, but there are places where the bright points fail to lie in a surface and where they are very fine: here the impression is that of a softer glow-like effect, like graphite. If set at such an angle as to get full reflection, the object does not show luster; but if the angle is changed just enough to escape the high reflection, it is lustrous. There is a smooth surface-phenomenon that lends an appearance of bulkiness to the whole. If one abstracts from this continuous glassy-like surface, one finds that beyond it is a mass that suggests considerable depth. This mass is both light and dark at the same time; typical luster.

(19) *Sub-metallic* (polished zinc, dull polished brass, bronze alloy, Hubnerite, bronzite, rutile). There are larger and smaller areas that are somewhat dimmed or obscured reflecting surfaces. These faces or planes give a dull or softened reflection. They are set amid non-reflecting parts. There are very small and rather widely separated bits of luster, which occur among the small areas of surface reflection and small bright points interspersed with dull, dead areas. These bits of luster are loose, glowy bits that lie behind the surface-markings. They are lightness and darkness together. At some angles the objects show almost no luster, while at others they show a great deal and the surface is much less apparent.

(20) *Splendent* (Helcioniscus exaratus, willemite, pectolite, chalcopyrite). Here we have the general effect of a much broken reflective surface with a variety of colors showing from the depths of the reflecting parts. In the truly splendent parts there is no luster.

(21) *Satiny-splendent* (chlorite schist). I have the "feel" that

if the surface of this object were smooth it would yield only shine. As it is, there are so many surface-cues that the glow is greatly reduced and thrown behind the surface. It is a very decent luster, except in the smoothest parts. The luster comes as a soft glow, tempered by an interfusion of darkness.

(22) *Mother-of-pearl* (pearl oyster, Nautilus, Avicula, mother of pearl, Patella magellanica). A soft uniform surface somewhat indefinite in localization. These objects, when set at the most favorable angle, do not show surface at all. One cannot say except in terms of adjacent cues where the visual presentation begins. There is a suggested depth; a soft, glowy mass of light that is also a mass of darkness.

(23) *Pearly— mother-of-pearl* (single valve of Unio; Haliotis asinina). Here we have the fairly uniform extended areas without definite surface that belong to *mother-of-pearl;* only in this case there is a certain definite hardening up of the whole. There are positive, although weak and indefinite, high lights that inhere in the body of the object, as in *pearly*. The soft lustrous mass is partly obscured by these bright portions.

(24) *Shining* (gold cloth, printed gold cloth, glazed tile, oligoclase, Ovulum gibbosum). In this class of objects the bright points are larger and more insistent than in the *silky* group. There is not much else here than the shiny, bright dots that form a definite hard and shiny surface. There is no luster.

(25) *Shining-glistening* (talc, gold cloth-2). The objects of this class show no luster. Between the bright points which come and go with movement are lighter and darker patches which lack a definite surface; but they are dense, compact, more like bulk, and the quality at any point is uniform.

(26) *Resinous* (copalite). I can find no luster here. There are surface reflections of light behind which, although not seen at the same place, is a semi-translucent bulky-mass.

(27) *Velvety* (milliner's velvet). An alternation of light and dark bits with very diffuse boundaries. They melt into one another so as to form a total mass. The whole lacks definiteness of surface; it has depth and is rather loose. In the lustrous parts there is no differentiation into light and dark bits, but simply the characteristic light-dark fusion.

(28) *Glistening-greasy* (elaeolite, satin spar, labradorite). These objects show small brilliant light points or very small patches that flash in and out with movement. They also present a fairly definite surface of lights and darks and colors all of which shade into one another more or less gradually. The *greasy* appearance seems to

belong to this melting together of color and tint within the slightly translucent total. There is no luster.

(29) *Sub-metallic-Greasy* (old pewter). The surface of these objects is soft and vague like the *sub-metallic*. The high lights come from inside rather than from the surface. The lights and darks blur into one another and give a greasy look to the whole. The luster is like all the other lusters I have seen.

(30) *Vitreous* (rock crystal, calcite, fluorite, calcite-2, phlogophite, calcite-3). The vitreous objects are transparent and highly translucent. In addition there are, generally, irregular and not very large reflecting surfaces where even brilliant high lights appear. Where there is a surface it is a hard, definite, shiny surface. There is never any luster.

(31) *Glistening* (micaceous hematite, glimmering flint, tinsel gold cloth, tinsel silver ribbon). With the glistening objects there are also many very tiny bright points, only now they are relatively further apart and much brighter. They are bright points set in a surface rather than forming the surface themselves. As the object is moved they flash out brightly. The characteristic thing is that the points gleam or shine out of an otherwise neutral surface. There is no luster.

These results show that our inference regarding the nature of luster is correct. They also encourage us to think that a phenomenological arrangement of lustrous objects, in one or more dimensions, is feasible. It could not, however, be made on the basis of these reports alone, nor is our supply of materials adequate to such a purpose.[11]

Besides these naturally lustrous objects we made up some synthetic metal preparations according to the directions given by Kirschmann.[12] H found that they give a metallic appearance, and under certain conditions show luster.

24 colorless mica plates. There is a very critical angle where I can get luster. It comes best if I have my back to the light and hold the mica out in front of me. This angle is just off direct reflection. Under all other conditions I get specular and surface reflection. The preparation is *metallic* for the most part, although when I have it in the position to show luster it is more nearly *sub-metallic*. There is always a surface glazing, and the luster shows through very weakly. At the angle at which it looks like new silver I should not call it lustrous at all.

24 sheets of colorless gelatine, 1 sheet of pale yellow under the top

[11]It is noteworthy that H found 7 classes (21 objects) which did not show luster.

[12]A. Kirschmann, Der Metallglanz und die Parallaxe des indirecten Sehens, *Phil. Stud.*, 11, 1895, 147 ff.

colorless sheet. As a whole it is *sub-metallic.* If it is turned directly to the light, the result is a bulky specular reflection dulled by very small surface bits. If it is turned slightly away from the source of light, the effect is that of imperfectly polished brass, not the satin-finished. It is much harder and brighter than the satin-finished brass, but not a perfectly smooth, highly polished surface. If it is turned slightly further from the light true luster appears beneath a nearly transparent surface of a glass-like nature. This glassy covering gives an appearance of density, of compactness, to the experience as a total that is not characteristic of luster. If one disregards this surface phenomenenon and looks beyond, one sees typical luster.

Same, with an orangish-red sheet in the place of the yellow. The preparation looks like polished copper. In this case the color is considerably stronger and its appearance is a better imitation of polished copper than the other was of brass. Also the glossy surface is more compelling; more difficult to abstract from. One can, however, look beyond it to the true luster. At the angle where luster appears the resemblance to a metal is very slight.

Same, green gelatine in place of yellow. Very much the same, except that it does not resemble any metal that I can call by name.

Same, weak purple in place of yellow. In this case the luster is a bit better, less obscured by the glassy cover. The luster at its best shows practically pure. It then resembles the *pearly* group.

Same, with a colored gelatine on top of the pile. It is like a highly polished metal with a coat of lacquer. But there is no true luster at all; it is simply a metallic-like appearance.

Orange gelatine, rolled about a stick to give several thin layers.[13] There is a very brilliant high light along the entire length of the cylinder. It seems to lie behind the surface, like the high lights of the sub-metallic group. The luster comes as a rich orangish glow that is all interpenetrated with darkness. The luster is indefinitely localized as beyond the surface light, is film-like in texture, and is the familiar light-dark fusion.

Exfoliated mica. This is a lustrous thing. I think it would be classed with the *metallic* group. The object presents streaks of high lights that are hard and impenetrable. Along some edges they break off quite abruptly, but on others they seem to grade off. Some of them are quite wide, others quite narrow. Between these high lights is the luster. It lies behind the surface markings: the lustrous areas have no surface at all. The luster is itself a soft, loose, light-dark, filmy thing.

72 cover glasses, .15 mm thick.[14] I can get these into such a position that they are lustrous; in that case the appearance of the total is that of *mother-of-pearl.* At other angles I get only brilliance and reflection. If I turn them to an angle that just eliminates direct reflection, the luster appears. There is always a glassy-like, hard, transparent overlay or surface.

Through the courtesy of Professors Louis A. Fuertes and E. B. Titchener we were able to supplement the laboratory collection by examining a

[13]For the rolled gelatines and the exfoliated mica we are indebted to Professor W. D. Bancroft, who used them in connection with a paper on Metallic Luster, which we shall mention below.

[14]Kirschmann reports that he was unable to get cover glasses of sufficient thinness to give luster; we were successful with glasses having a thickness of .15 mm.

number of additional objects of various kinds.[15] We found further confirmation of H's observations: many objects commonly regarded as lustrous proved to have no luster; and, when luster appeared, it was always the same.

Indian pheasant, head. This is metallic. The color ranges from green to blue green, to violet red, to copper red, to yellow green, to blue green. Everywhere there are brilliant surface reflections in rather large areas, very smooth and uniform. At the places of the most brilliant reflection there is no luster. In other parts, where it is not so brilliant, there is luster. The luster lies beyond the adjacent surface cues. It comes as a glow. It is the same sort of phenomenon that I have always found luster to be.

Bronze grackle, head. The reflections are not quite on the surface; there are very fine dashes in front of the high lights that partly obscure them and give a semblance of luster. But these brightest points are not lustrous. Just off the high lights, however, there is luster that comes from behind and shows through a glassy-like surface. As a whole the object is *sub-metallic.*

Bronze grackle, back. The sub-surface reflection is interrupted by very fine dashes and points again. They are so fine that any one is difficult to perceive separately, but the total effect is one of separate, discrete lights and darks. If one retreats to a little greater distance, some areas become truly lustrous: the bright and dark parts fuse into a film-like mass. This also is *sub-metallic.*

Lusterware. We examined a number of pieces of lusterware and found no luster except in the case of black luster. Pink luster shows no trace of luster: the impression seems to come from the glaze. Bronze luster is highly metallic in appearance, showing sharply defined, brilliant high lights, but no luster. Crown Derby, in which the blue is commonly spoken of as lustrous, was also lacking in luster when examined critically. Black lusterware has a very good luster, which belongs to the phenomenological group *pearly.*

Majolica. We found this to be entirely without luster, although the glaze makes it reflect light in brilliant spots of high light.

Glazed Earthenware. The same thing can be said of glazed earthenware.

Old Pewter. Old pewter shows a good deal of luster. The surface is not definite enough to make this luster appear metallic, and the lights and darks blur into one another in such a way as to make it look greasy. It is classified phenomenologically as *sub-metallic-greasy.*

Bronze Alloys. Japanese bronze alloys are beautifully lustrous, and show the phenomenological characteristics of the *sub-metallic* group.

Old Copper. Old English copper is likewise lustrous, and is classified as *metallic-satiny.*

Old Gilt. Old gilt, English and Chinese, is lustrous. These objects also belong to the *metallic-satiny* group of our classification.

Lacquer. Ordinary lacquered surfaces give no luster but only deep, mirror-like reflections; we were able to verify our observations on old Chinese black and modern Japanese red lacquer. An old Korean image, which had been treated with several coats of gold lacquer gave, on the

[15] Of the other objects mentioned below, we are indebted to Professor L. A. Fuertes for the birds, and for the rest to Professor E. B. Titchener. Professor Fuertes showed us a large collection of birds, and those which we report on were selected as typical.

other hand, a very marked luster and showed the phenomenological characteristics of the *sub-metallic* group.

Polished Horn. Polished horn has patches of pure luster. We were unable to classify the object as lustrous with respect to its appearance as a whole; the lustrous patches showed as 'reduced' luster, with no relation to a surface. This result may have been due to the conditions of observation (horns fixed to wall).

Spanish Leather. Spanish leather is very lustrous under the right conditions. The luster is so nearly 'reduced' luster that the leather about defies classification as an object. *Sub-metallic-satiny* is perhaps the most nearly adequate classification.

Two incidental observations of 'reduced' luster were made. The reflection of a polished mahogany table in the round base of a bronze-luster pitcher gave as good a 'reduced' luster as any of our synthetics. A smaller patch appeared on the majolica, at a point where one would have expected to find a high light. Close examination showed a thin film of dust on the surface of the glaze at this point; when the dust was brushed off the luster disappeared, and in its place was a brilliant high light.

(F) Miscellaneous Experiments

(1) Stereoscopic luster

Even since 1851, when Dove made the first relevant observation, it seems to have been taken for granted that the stereoscopic combination of a figure drawn in white lines on a black background with another drawn in black lines on a white background gives a graphite-like luster.[16] We repeated the observations, with every expectation of confirming Dove's result, because they promised to give us another mode of synthesis. But we found, to our surprise, that there is no such thing as stereoscopic luster. What is typically seen is the grey side of a tridimensional figure overlaid by 'glass'.

Our experiments were performed with a Zeiss Precision Stereoscope; as stimulus we used Titchener's Series, 32.[17]

(H, TS 32, 3) A shiny grey fills the planes of the sides of the figure. A dense, hard, compact, translucent grey, which appears shiny and rather thin against the black that lies behind it. The main thing is that one sees through the grey which is localized in the plane of the object. It seems to be this seeing-through the grey that gives the grey its appearance of being shiny. The dense, glass-like character of the perceptive experience, its smooth uniform consistency, seems to give the shiny character. The grey, as such, is not shiny; it is not loose or soft, nor in any way like a filmy thing. It is uniform; *i. e.,* it is grey and not dark-light.

[16]H. W. Dove, Die Ursachen des Glanzes, *Bericht ü. d. zur Bekanntmachung geeigneten Verhandlungen d. kgl. Akad. d. Wiss zu Berlin,* 1851, 225 ff.

[17]Stereoscopic Slides, Titchener's Series, C. H. Stoelting Co., Chicago, Ill.

(B, TS 32, 1) There is no luster here. The planes of the figure appear shiny, hard, glassy grey. This surface resembles the surface of many lustrous objects, particularly the surface of shells; but there is no luster. Sometimes the grey becomes pushed back and is seen as a grey bulk behind the sides of the figure, which become colorless 'glass'. The grey is always grey; never luster. Taken common-sensibly, the object has a shine, but nothing of the glowy nature that one sees in lustrous objects.

Brewster in 1855 declared that, while luster appears stereoscopically with object-pictures as stimuli, the stereoscopic combination of total black and white fields does not produce luster.[18] We tried this procedure also, using a card 33 x 9 cm, half black and half white, which gave an uncontoured field. Brewster's statement is correct: under these conditions there is no glaze or shine (the phenomenon that Brewster took for luster), to say nothing of luster proper. Nor can the appearance of the field be changed by any effort, on the part of *O*, to put himself into an object-attitude.

(H, Uncontoured Field, 1) The total field is grey. This grey forms a surface which is very compact; very much a surface. The irregularities of the card make a great many specks and spots. It is a very dull thing; certainly not lustrous.

(B, Uncontoured Field, 1) This slide gives nothing even suggesting luster. It is simply a rough grey surface. The experience has no depth or shine.

We then tried to bring back the glaze by a slight decentering of the stereoscopic slide; the field now showed a middle area of grey, bounded to right and left by strips of black and white (or white and black); the middle area was contoured. We found that the presence or absence of glaze (Dove's stereoscopic luster) depended on the attitude of the *O*.

(B, Tripartite Field, 1) The visual field has three sharp divisions, on the extreme right there is a narrow, vertical band of black; on the extereme left a similar band of white; between the two there is a wider band of grey. If I sit down to the observation casually, the grey strip is seen as very shiny and very lively. If I examine it more closely the grey is forced back a bit, and is seen through a bulk of 'glass'.

I can, however, sit down with a conscious set to observe a sheer visual extent, and the results are quite different. There is no shine, no liveliness, no 'glass'. I see three vertical strips, all of them having a definite, dead, dull surface. The surface-effect seems to be given by the irregularities and roughness of the stimulus.

(H, Tripartite Field, 1) If I take it casually it is a contoured grey field which is a shiny, glassy, glistening grey. If I look at it with a definite 'set' to disregard the contours, and take it simply as a visual extent, it becomes a dull, dead, grey surface. The rough texture of the slide is very apparent under this attitude.

[18]Sir David Brewster, Report of the Meeting of the British Association for the Advancement of Science at Glasgow in 1855, *Athenaeum,* 1855, 1120.

The same effect of attitude may be noted even with the ordinary slides (outlines of tridimensional objects).

(B, 12-sided figure, 2) With a great deal of difficulty, after looking at the slide for a relatively long time (5 or 6 min.), I was able to get the same results. All trace of 'glass', shine, and tridimensionality disappeared, and I had a light grey, perfectly dull, visual extent, filmy in texture. This appearance lasts only a very brief period at first, but becomes more stable with practice.

I found the observation easier if I shaded the white drawing. When the whole became darker, the non-object phase was more stable.

We hope later to report systematic observations on the effect of attitude.

Finally, since the luster reported as seen in the stereoscope under Dove's conditions has been called 'graphite' luster, we asked H for comparative phenomenological accounts of the surface of a piece of graphite and of the stereoscopic slide.

(H, Graphite, 1) There is a great deal of surface brightness and darkness, but there are lustrous portions as well. At the edge of one of the areas of luster there is a bit of surface-brightness which has diffused all through it a quality of darkness, the whole giving a soft experience like a film except that the depth is more definite. It is a soft mass that is both bright and dark at the same time; the peculiar interpenetration of light and dark in which both retain their individual character as light and dark and yet neither is perceptively discrete. At its very best, however, there is no front surface where the object is lustrous. These lustrous areas are really very small and are interspersed irregularly all over the object, showing between the bright reflecting surfaces and the darks. Comparing it with the stereoscopic slide, I should say that the graphite has many shiny, smooth portions that form a bright, shiny, smooth surface very much like the planes of the figure in the stereoscope in quality, smoothness, density and reflecting shine. But the stereoscopic figure, on the other hand, shows no luster.

(2) Painted luster

We were fortunate in being able to examine two paintings of lustrous objects, a satin dress in a portrait by Professor Olaf M. Brauner, and a pewter platter in a still-life by Professor Christian S. Midjo.

(H) The lustrous effect of the dress is unmistakable: the artist has succeeded in his portrayal of a lustrous fabric. Viewed closely, the picture presents the usual surface-effect, with the many colors fairly sharply set off from one another; they lose their specific hues and the definite surface vanishes. What one now has is a much softer effect, with the lights and darks running into one another. The texture ranges from a fairly high density with the high lights to the softer greys which have the appearance of shadows. These greys are not definite surface grey, nor are they the luminous, glowy mass of luster. Somehow the soft grey and the high lights combine to give the impres-

sion of luster; one knows that only a lustrous object could present this combination. At the same time, the typical commingling of light and dark in a filmy bulk is not present.

(B) The impression of luster is perfect, although there is no trace of the light-dark luminous film that one sees in lustrous objects. The meaning of the object, the distribution of the lights, and particularly the way in which the lights and darks run into one another and produce the effect of softness, filminess, seem to be the cues for the unmistakable impression of luster.

(H) As an object, the platter is certainly lustrous. A lustrous plate. I believe there is a kind of greyness that will not be surface—it simply defies surface. It is the same sort of thing that I saw in the dress of the other picture. This softness and greyness is something like luster, and it borrows from the high lights, and makes the whole thing look lustrous. I find no luster of the kind we have described in lustrous objects.

(B) It is a beautiful representation of a lustrous object. I do not find luster, however, if I examine it carefully. The distribution of lights is like that of a pewter platter, and in the center (where one would expect to find the best luster in the real object) there is a large area of non-surface, shadow-like grey. Seen closely, this area is made up of a great many different shades of grey all blending and shading into one another. From farther away it takes on a soft, filmy appearance; it has not the true quality or texture of luster.

(3) Casual Observations

When once one has become familiar with 'reduced' luster, one sees it frequently in everyday life. We give a few instances reported by our *O*s.

(B) On three occasions I have seen luster when the lights of a car coming toward me during a heavy snowstorm could not be seen directly. If the flakes are very fine, the diffusion of light makes a lustrous mass of indefinite dimensions.

(T,[19] confirmed by B) I have often seen 'reduced' luster on foggy nights, when a car is coming toward me from the other side of a slight grade, and our positions are such that the crest of the rise prevents my seeing into the headlights and I get only the diffusion of the light through the fog.

(K, confirmed by F, B, and G) Standing in a west window on the third floor of Morrill Hall as the sun was setting, I saw the entire valley filled with a lustrous golden haze (May 10, 1926, 7 P. M.).

(B) One night, as the full moon was rising, I was in such a position that the disc of the moon was just out of sight below the crest of a roof. A large circle of luster appeared above the roof; its localization very indefinite; I could only say that it was higher up than the roof because I knew the source of it; I could not say that it was nearer or farther than the roof.

(F, confirmed by T and B) I came into a warm corridor from the cold out-of-doors and, as usually happens, my glasses became fogged. As I walked along the dark corridor toward a flood of bright light that came from a lighted room opening off the corridor, I found my-

[19] (T) Professor E. B. Titchener.

self walking into a 'bank' of luster which completely filled the visual field.

(H, confirmed by B) I sat in a very dimly lighted room with a piece of cotton-batting in my hand. As I looked at it it suddenly struck me that it was as lustrous as anything I had ever seen. It looked very much like the luminous paint that I had worked with some time before.

(B, confirmed by F and K) The diffusion of the light from a street-lamp (itself hidden from view by a branch of a tree) seen through a cloud of dust raised by a passing car gave a splendid luster.

(G) Historical

Beginning with Dove in 1851, many psychologists have tried to formulate theories of luster based on the physical and physiological conditions of its occurrence. We are not, in this paper, interested in establishing a theory of luster. Our interest in luster lies wholly in phenomenology and experimentation; it is therefore from those two angles that we shall discuss the history of our problem.

(1) Phenomenology

Hering was the first to attempt anything like a phenomenology of luster. [20] On a lustrous surface, he declares, there is always an irregular alternation of light and dark spots; but this alternation of light and dark is not itself the essential characteristic of luster, since it may also occur on non-lustrous surfaces. When one sees luster, one sees the surface of the object in the body-color of the object and, in addition, a surplus of 'accidental' (zufälliges) light or dark quite apart from the body-color of the object. This 'accidental' light or dark may be localized on or before the surface of the object, or may be seen as coming from the depths of the object. Luster always appeared to Hering as a 'split (Spaltung) of sensation,' the body-color of the lustrous object and the surplus light or dark seen as independent of the body-color.

Katz is in general agreement with Hering: "luster appears only in an object, and surpassing the color of the object in brightness is apprehended as light which does not really belong to the color of the object."[21] He has, however, carried his observations farther than Hering, and his conclusions as to the texture of luster and the basis of differentiating the different

[20] E. Hering, Der Raumsinn und die Bewegungen des Auges, in Hermann's *Handb. d. Physiol.*, 3, 1879, 575 ff.

[21] Katz, *op. cit.*, 21.

kinds of luster are in line with our own. Although luster usually occurs in connection with surface-colors, Katz says that the lustrous area itself never appears as a surface-color but is much more like a film-color. It is this filmy appearance of the lustrous areas which distinguishes luster from other visual phenomena in which, because of strong partial illumination, light seems to lie on the surface-color. In these cases of non-lustrous, partially illuminated surfaces, the brightly illuminated areas appear also as surface-colors. On the other hand, although luster is very similar to film-color, Katz observes that film-colors are never lustrous. With respect to the different kinds of luster, Katz says: "metallic luster appears only in objects which have the specific appearance of metals. The peculiarity of metallic luster lies less in the isolated color-impression of the lustrous parts than in their combination with the non-lustrous parts of the metals. Analogously, the special character of the other kinds of luster is to be sought in the appearance of luster on a background of this or that peculiar structure."[22] For Katz, then, as for us, the different lusters are not different in themselves, but gain their specific character from the objects to which they are referred.

(2) Experimental

It seemed to us worth while to repeat some of the experiments by which luster has been synthetically produced, in order to see whether these synthetic lusters are true luster or only something closely resembling it. H and B made the observations, and the findings in the following paragraphs are drawn from their reports.

The first important experiments are those reported by Dove in 1851.[23] We have already spoken of the experiment in which he used black and white stereograms.[24] In the same article Dove says that, when he examined stereoscopic drawings of truncated pyramids with the cut surface of one pyramid colored blue and that of the other colored yellow, at the moment when green appeared, in the transition from yellow to green, the cut surface appeared to be covered with a 'gloss'—a term which for him is equivalent to luster. We find that when green appears the mixture is filmy and has, therefore, the luminous character of films. When the colored areas are small, the peculiar ap-

[22] *Ibid.*, 27.
[23] Dove, *op. cit.*, 255 ff.; and later.
[24] See p. 162.

pearance of the mixture is difficult to identify; but with larger areas one sees the filming very easily. It is this filming of the colors, with its accompanying luminousness, that gives the meaning of luster. Dove says further that, if one looks at this mixture through a violet glass held in front of both eyes, the cut surface shows like polished metal. We found that the effect of the glass is to reduce the hues to black and white, which give the same appearance as the original black and white figures.

Brewster, in 1885, suggested an experiment which tells against Dove's theory; this has already been mentioned above.[25]

In 1857 Dove wrote that, when he looked at a blue picture outlined on a red field, holding a red glass before one eye and a blue glass before the other, he first saw the picture as black on red.[26] Suddenly the blue stood out, and at that moment the whole was vividly lustrous, "as if the colors were put on in porcelain."[27] This holds for red and green as well as for blue and red. We have used all four colors. Taken casually, the colors, it is true, look like porcelain at the moment of fusion; but there is no trace of luster. The whole field becomes softer, not soft enough to be filmy but more like a soft surface;[28] it is labile, constantly shifting, and shows areas of dark and light without sharp boundaries. It offers a close approximation to luster, but differs in two respects; it is too surfacy in texture, and the darks and lights are perceived as definite spots.

Paalzow, in the same year, proposes a way in which the appearance of luster can be synthetized monocularly.[29] An outline figure is drawn in dull white on a blue glass. As one looks at the figure, one sees it colored yellow by contrast. If one cuts down the amount of light shining through the glass, by holding a white paper behind it, and moves another narrow white strip rapidly back and forth between the eye and the glass so that now the contrast color and now the white appears, the picture takes on a metallic luster. Our observations show that the figure takes on a metallic look; it could easily be called golden. It is too hard to be luster, however, too dense, too compact. There is not the mobility of luster; it is bright without

[25]Brewster, *op. cit.*, 1120. See p. 163.

[26]Dove, Ueber Binocularsehen durch verschiedene gefärbte Gläser, *Pogg. Annal.*, **101**, 1857, 147 ff.

[27]*Ibid.*, 150.

[28]M. F. Martin, Film, Surface, and Bulky Colors and Their Intermediates, *Amer. J. Psychol.*, **33**,1922, 451 ff.

[29]A. Paalzow, Ueber subjective Farben und die Enstehung des Glanzes, *Monatsberichte d. kgl. preuss. Akad. d. Wiss. zu Berlin*, 1857, 390.

being motile. And there is no dark-light interpenetration; just brilliance. The picture is seen as shining like a highly polished metal surface.

In 1861 Dove cited two experiments to show that the idea of solidity is not essential for the perception of luster.[30] The first consisted in looking at a white square on a black background through differently colored glasses held in front of the two eyes. In the transition from one color to another there was an intermediate stage of luster. In repeating the experiment we used combinations of blue and green, and blue and red.[31] At the moments of shift, the look of the white square suggested luster; it was soft, with a suggestion of depth; there was a speckling of very minute dark and light bits; and there was a certain lability about it. There is, however, too much body in the experience; the dark and light are too distinctly a speckling; and the lability is a lability of the whole mass rather than an inner lability. The second experiment Dove describes as follows: "I . . . . . placed a deep blue glass on the diameter of a circle which was one-half red and one-half white, in such a way that the reflected white surface was seen as blue by refraction. The appearance reminds one strongly of violet satin, even with monocular vision." [32] It is possible to adjust the angle of the glass to such a position that the visual appearance is like that of a violet satin, but the whole is more bulky than a true lustrous satin. It has a bright rather than a glowy look; more nearly a *shine* than a luster. The liveness is, again, a liveness of the whole mass rather than an inherent liveness.

In the same year Rood published a long series of experiments in which he imitated the appearance of different metals by the combination of various colors and materials in the stereoscope.[33] In repeating these we worked with the Zeiss Precision Stereoscope, and, following Rood's directions, we made up our colors from the Chevreul circles. In the first series we combined different colors with tin-foil or gold-foil. According to Rood, orange-yellow no. 4 of the first circle in combination with slightly crumpled tin-foil looks like gold leaf; orange of the first

[30]Dove, Ueber den Glanz, *Monatsberichte d. kgl. preuss. Akad. d. Wiss. zu Berlin,* 1861, 522 ff.

[31]For our colored glass we used Wratten Monochromatic Filters manufactured by the Eastman Kodak Company of Rochester, N. Y.: blue no. 6, green no. 1, and Red no. 5.

[32]*Ibid.,* 525.

[33]O. N. Rood, Upon some Experiments connected with Dove's Theory of Luster, *Silliman's Journal,* Series II, 31, 1861, 339 ff.

circle with tin-foil, like copper; nos. 14 and 15 of the red-black scale with tin-foil, like bismuth; and ultramarine blue with tin-foil like foliated graphite. Gold-foil with green of the first circle resembles murexide; and gold-foil with ultramarine blue cannot be distinguished from the combination of ultramarine blue with tin-foil. In all of these mixtures the resemblance to a specific metal is very striking, and luster is present. The luster is the same in every case; the specificity of the metallic character derives from the color rather than from a difference in the luster. As a variation of these experiments Rood replaced the tin-foil by dark grey paper, but was unable to get the metallic effect. He attributes the failure to the fact that "the well-known texture and appearance of the paper forcing itself upon the attention precluded the idea of anything metallic."[34] We should explain it in a different way, referring it to the absence of the areas of high light and deep shadow characteristic of the metallic appearance. To overcome this difficulty, Rood proposed combining the colors with photographs of crumpled tin-foil; he found the metallic appearance of these slides but little inferior to that of the real tin-foil. We found that the imitations are surprisingly good, but that they lack luster. Our *O*s were able to say whether they were looking at a picture or at a real tin-foil preparation by the 'flatter,' 'drabber,' 'lusterless' appearance of the photographs. A second means of getting metallic imitations without using lustrous material in the slides Rood describes as follows: "The surface of a plate of brass one inch square was polished and then rather heavily scratched by a coarse file. Into the scratches a small amount of yellow or white oil paint was rubbed, and upon this prepared surface dark grey or black paper was laid and the whole submitted to the action of a press as in copper-plate printing. By this means a drawing of a scratched metallic surface was transferred to paper. These markings serve also to enable the observer much more easily to direct his attention simultaneously to the two impressions presented."[35] We have followed these directions as carefully as possible, and found no metallic appearance when drawings prepared in this way are combined with yellow papers. Neither was there any luster. In a final series of stereoscopic experiments Rood reports that he was able to produce the metallic imitations by combinations of various tints made up according to the Chevreul circles. For examples, grey

[34] *Ibid.*, 340.
[35] *Ibid.*, 340.

no. 18 with no. 4 of the blue-violet and black scale looked like slightly tarnished lead; the appearance of antimony was given by no. 1 of the blue and black scale with grey nos. 18 to 20, or by no. 17 of the blue-black scale with white. We did not find these imitations very convincing, and in all cases they were without luster.

In the same paper Rood reports several experiments in which he succeeded in producing luster monocularly. (1) "White smooth drawing paper was rubbed over irregularly with a brush slightly moistened with a weak wash of India ink or lamp-black; when dry, another wash of a deeper hue was applied as before, care being taken to leave many small spots untouched. The final wash was laid on with pure black. If the brush is kept nearly dry and passed only lightly over the paper, it is easy to obtain a surface bearing some very distant resemblance to the minerals above mentioned; it is of course without luster. When these papers were held behind discs of ultramarine or orange-tinted paper, from which equal alternate sectors had been removed, and which were revolving at such rates that their surfaces seemed uniform, or at lower rates, they often appeared, to the single eye, highly lustrous."[36] Working in a dark room, we were able to adjust the angle of illumination to give us luster. We could, however, get the luster quite as well with a plain dark grey background as we could with the prepared paper. (2) "If a piece of this peculiarly blackened paper one half of an inch square is placed in a blue field (rather light ultramarine paper) and is steadily regarded for some minutes by one eye, it assumes a red-orange hue and appears suspended over the blue paper and nearer to the eye than the latter; at the same instant it appears lustrous like crumpled mica."[37] Our *O*s reported that the square becomes filmy and luminous, but not lustrous. This is also true of a further variation of the same experiment, in which a sheet of the prepared paper is brightly illuminated and held so near one eye as to produce indistinct vision. (3) A roll of this paper is illuminated on one side and is looked at through a plate of colored glass; "after a few seconds it appears lustrous, resembling a roll of polished zinc which has been irregularly and deeply corroded by an acid."[38] We looked at a roll of the paper through a blue glass. The paper smooths out and looks somewhat metallic; the surface becomes more

[36] *Ibid.*, 343.
[37] *Ibid.*, 343.
[38] *Ibid.*, 344.

uniform; differences in brightness become something like the sub-metallic reflections seen on dull metals; the texture becomes softer, but not soft enough to be luster. The roll of paper when viewed under these conditions has a slight resemblance to the class of objects we have called sub-metallic, but it shows no luster.

The following year, 1862, Rood wrote of a method of producing luster detached from any material surface by the convergence of the ocular axes.[39] "An aperture, one-half inch in diameter, is cut in a blackened piece of cardboard, and across the opening a sewing-needle is fastened by a little wax. The card thus prepared is held at a distance of six or eight inches from the eyes, while behind it, at a distance of a foot or more, a sheet of white paper is placed beside one which is blackened. The cardboard is held in such a manner that the aperture appears black to one eye and white to the other; the ocular axes are then converged on the needle, which is attentively regarded. The open aperture now assumes a lustrous appearance, and one is tempted to believe that it encloses a plate of very clear polished glass; but as an instinctive search for minute particles of dust on this imaginary surface fails to detect them, the idea is gradually forced on the mind that it is the air itself which has become lustrous."[40] Under these conditons, what one sees is the 'glassy-sensation,' not luster.[41]

In the same year Wundt published the results of two new experiments on luster.[42] (1) "Reflect onto a colored object the image of an object of another color, by means of a glass plate held at an inclined position between the two objects, in such a way that the reflected image lies slightly behind the directly seen object. If the colors contrast, and if the objects are definitely bounded, one gets the monocular impression of a lively luster."[43] We used a piece of blue paper, 10 cm. square, reflected onto a piece of yellow paper of the same size. The yellow paper was partially shaded. At first look there is something a little like luster in looseness of texture and softness. The area is not uniform in quality; there are splotches of blue

[39]Rood, On some Stereoscopic Experiments, *Silliman's Journal,* Series II, 34, 1862, 201 ff.

[40]*Loc. cit.*

[41]E. F. Möller, The "Glassy Sensation," *Amer. J. Psychol.,* 36, 1925, 249 ff.

[42]W. Wundt, Ueber die Entstehung des Glanzes, *Pogg. Annal.,* 116, 1862, 627 ff.; and later.

[43]*Ibid.,* 628.

and yellow not definitely set apart. There is no light-dark filmy-bulk. With continued fixation there is a marked change in the perception; the object becomes predominantly yellow in quality and seems to have a sheen or hard, surfacy, shine like a sheet of gold. There is still no luster, but only a distinctly metallic look. (2) Place a colored paper, in which a small window has been cut, on a colored glass plate of a different color. Look through this window at a perspective drawing on a white background held at some distance behind the glass plate. The window seems to have a very lively luster, whether one looks with one or with both eyes. Using a green paper with an oblong window 3½ cm. x 1 cm. on a cobalt blue glass, and looking at the drawing on one side of a stereoscopic slide, Titchener's Series no. 14, we found that the window appears filled with 'glass.' One can, by taking the attitude for it, push this glass back to the drawing and see the drawing as bright and shiny; it is never lustrous.

Kirschmann in 1895 made some preparations for synthetic lusters; these have already been reported upon.[44]

In 1925 W. D. Bancroft and R. P. Allen published the second of two papers on metallic luster.[45] In the first they reviewed the psychological and physical theories of luster already in print; in the second they advanced their own theory and reported a great many experiments to support it. We have not had sufficient time to repeat all of their work, but they have been good enough to let us examine certain of their preparations.[46] In some cases we agreed with them in finding these objects lustrous; in others we disagreed. They have been content to accept a 'metallic appearance' as metallic luster; we cannot reconcile this conclusion with our own observations. One of the experiments which Bancroft emphasizes is the 'synthetic pie plate;' "a cardboard disc, 20 cm. in diameter, had two black quadrants painted on it, radiating from the center with a white rim 2 cm. wide on the outer edge. The white quadrants had a black rim 2 cm. wide on their outer edges. When this disc was rotated at about 150 R. P. M. and was illuminated by intermittent light supplied through a sectored disc revolving synchronously, the black and white disc became exactly like an aluminum pie plate."[47] We, too, find that the disc looks like

[44]Kirschmann, *op cit.*, 147.

[45]W .D. Bancroft and R. P. Allen, Metallic Luster II, *J. Physical Chem.*, 29, 1925, 564 ff.

[46]We did not read Professor Bancroft's and Dr. Allen's paper until our own observations were concluded.

[47]*Ibid.*, 581.

an aluminum pie plate; but we are equally sure that there is no luster. Another device, particularly interesting to us, was a "small box fitted at the open end with a sheet of red gelatine, 6 cm. square. A 25 watt Mazda lamp was placed inside the box and ground glass for diffusing the light was set behind the gelatine. In front of the gelatine was placed a black wire gauze with meshes 2 mm. square. If one looked at this arrangement from a distance of about five feet, it appeared very lustrous. When closer, the meshes of the screen could be seen too easily, while they were not visible enough if one stood too far away."[48] A variation of this experiment is to spot the gelatine with black dots: "a sheet of yellow gelatine, 10 cm. square, backed with translucent paper, and illuminated strongly from the rear will not look metallic at a distance of three or four feet. If the surface is changed by putting on a number of small black dots with India ink, then it begins to appear lustrous and seems like a piece of brass."[49] We tried this procedure, flicking the small dots on with a stiff brush, and found that in order to get a truly lustrous appearance it is necessary to have the stimulus slightly out of focus. If we looked at it in focus, the darks and lights were seen as separate bits.

Conclusions

(1) The phenomenon of luster is the same under all conditions of its appearance.

(2) Qualitatively, luster is an intermingling or 'sifting-together of light and dark'—'light and dark seen in the same place.'

(3) This paradoxical mode of appearance depends upon the extreme lability of luster.

(4) The texture of luster is a bulky-film.

(5) While in ordinary life luster is, by the untrained *O*, always seen on objects, and is therefore connected with the perception of surface, we have been able to produce experimentally a pure or 'reduced' luster without surface which, once experienced, can also be found under non-experimental conditions.

(6) It seems probable that lustrous objects may be classified phenomenologically into one or more finely intergrading series. Further materials and observations are, however, necessary before such series can be established.

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[48] *Ibid.*, 582.
[49] *Ibid.*, 579.

NOTES

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THE GLASSY SUBSTANCE

(1) When we were occupied in the Cornell laboratory with the glassy substance,[1] I was sometimes asked if, after all our practice, the perception might not perhaps become obsessive. I can now say that it does not. I see the glass far more frequently than I did before I took part in Dr. Moeller's investigation; but it serves simply to enhance or maintain tridimensionality; and days go by on which I am not aware of it.

(2) I have often amused myself by trying to make out how many blocks or chunks of the glassy substance I could see before me in a complex landscape. My usual standpoint is at an upstairs window which looks out through scattered garden-trees to a tree-lined road; but any complicated landscape will do, provided that its separate objects come fairly close up to the eye. One may frequently see three blocks, less frequently four, very occasionally five. Fixation must ordinarily be successive; if one fixates the boundary between two successive blocks, one is likely to see it as set in a matrix of glass which extends indefinitely to either side.

(3) It is not difficult to fill a portion of the sky with glass. Thus, if one stands back from an open window which gives upon a cloud-flecked sky, the square of sky—especially if the lower part of the picture shows trees or buildings—looks definitely glassy. I have once seen the whole sky filled with glass. It was early on a summer evening; I was looking south into an intensely blue sky, over a belt of trees whose tops made a rather high horizon; and I suddenly realised that the whole field of vision, above, below, and as far as I could see to right and left, was solid with glass. I have never been able to repeat this experience.

(4) I once had the opportunity of comparing the glassy substance with the memory-color of real glass. I had been looking out of window, as described above, and as I turned away I saw on the floor a small triangular piece of glass. It struck me that the two glasses were very closely alike, and I looked out of window again to make sure; the only difference was that the real glass was bounded by two lines of high light, while the glassy substance out-of-doors had dark boundaries. Then I stooped to pick up the bit of real glass, and found that it was an open safety-pin.

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[1] E. F. Moeller, The "Glassy Sensation," *Amer. J. Psychol.*, **36**, 1925, 249 ff.

EMPIRICAL AND EXPERIMENTAL PSYCHOLOGY

Professor Carmichael objects to my use of the term 'empirical psychology.' He does not discuss the question whether my characterisation of empirical psychology is correct; his objection is rather terminological.[1]

Professor Carmichael himself distinguishes three uses of the term. We may, however, rule out the second of these uses, since it involves a confusion of 'empirical' with 'empiristic.' It is true that the translators of Klemm's History of Psychology refer to Helmholtz' "empirical view;" Klemm, however, writes "empiristische Ansicht."[2] In any case, 'empirical' cannot be the adjective both of 'experience' and of 'empiricism.'

Two uses remain. The first use, which Professor Carmichael regards as sanctioned by history, makes 'empirical psychology' the same thing as 'experimental psychology.' The other, which is mine, characterises empirical psychology as a rationalisation of mind-in-use; Professor Carmichael proposes to call this 'rational psychology.'

A system of psychology is 'rational' if and in so far as it is based upon a priori considerations. Professor Carmichael quotes Maher to the effect that in rational psychology "the truths . . .enunciated are reached, not by direct experience, but by reasoning from the conclusions established" in phenomenal, empirical or experimental psychology. If he had read a little further, he would have found that rational, metaphysical or philosophical psychology "seeks to learn what may be gathered by the light of reason regarding the nature, origin, and destiny of the human soul;" and if he had continued reading, he would have discovered what sort of thing this 'light of reason' is.[3] Wolff is more outspoken: "Psychologia rationalis tradit ea, quae a priori de anima innotescunt."[4]

Brentano's psychology, according to Professor Carmichael, is a rational psychology. Brentano ought to have known. He calls his book, on the title page, "Psychologie vom empirischen Standpunkte;" he writes in his preface: "Mein Standpunkt in der Psychologie ist der empirische; die Erfahrung allein gilt mir als Lehrmeisterin."[5] There are members of Brentano's school who think—as I myself do not—that aprioristic elements have slipped into Brentano's exposition;[6] no one hitherto, as far as I know, has ventured to dub his psychology as a whole 'rational.' Klemm, of course, treats him as an empirical psychologist.[7]

What, then, of the historical identification of 'empirical' and 'experimental' psychology? Wundt ought to have known; and Wundt regards experimental psychology, a science wrought out on the analogy of experimental physiology, as a new discipline, breaking away in protest from

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[1]L. Carmichael, What is empirical psychology? *Amer. J. Psychol.*, **37**, 1926, 521 ff.

[2]O. Klemm, *Geschichte der Psychologie,* 1911, 346; cf. 42.

[3]M. Maher, *Psychology, empirical and rational,* 1900, 5, 459 ff.

[4]C. Wolff, *Psychologia empirica,* 1732, 4. Cf. *Psychologia rationalis,* 1734, Praefatio: "nemo . . . hactenus philosophorum ex essentia animae rationem a priori reddere conatus est, cur" etc.; and many other passages.—Professor Carmichael cites Wolff in the ablative case, forgetting that there is on the title-page an *autore;* he also writes *contentur* for *continentur.*

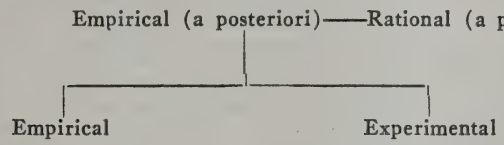
[5]F. Brentano, *Psychologie vom empirischen Standpunkte,* i., 1874, v.

[6]Cf. my review of O. Kraus' edition, *Amer. J. Psychol.*, **36**, 1925, 303 f.

[7]*Op. cit.,* 87 ff.

the empirical psychology which preceded it.[8] I have already written so fully of Wundt's attitude that I need not repeat myself here: let me insist only that I am not the inventor of the antithesis empirical-experimental; I take the antithesis as I find it, in the Brentano and Wundt of 1874.

So we have the following table:



and I see no reason for confusion. Experimental psychology is an a posteriori psychology; empirical psychology may, and does, employ the experimental method: but a student of the history of psychology who should be confused by these facts would, surely, be a very superficial student! At all events, the important thing, it seems to me, is to show wherein the two current psychologies differ; and that I have tried to do.

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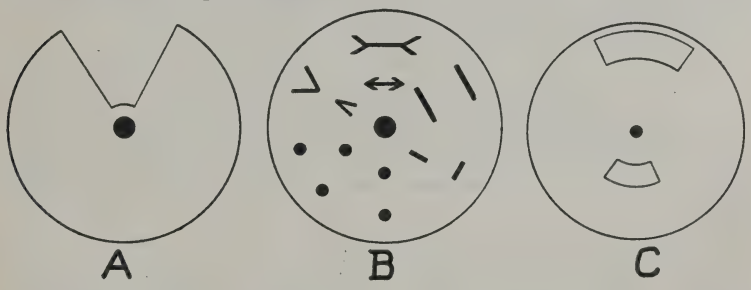
A SIMPLE DEMONSTRATION OF MOVEMENT BY DISKS

Beginning with Wertheimer[1] the phi phenomenon has been the subject of a number of researches, and has assumed such importance that a practical demonstration of it has become essential in the lecture room. For this reason we are communicating this description of a simple way to demonstrate the phenomenon.



The three disks in the figure are all of black cardboard. Disk A is the cover disk with a sector cut out, and in use lies upon disk B. Disk B furnishes the figures desired and these are cut out of the disk as

[8] W. Wundt, *Beiträge zur Theorie der Sinneswahrnehmung,* 1862, vi; *Grundzüge der physiologischen Psychologie,* 1874, 3.

[1] M. Wertheimer, Ueber das Sehen von Bewegung, *Zeit. f. Psych.,* 61, 1912, 161-265. Other references are given by H. G. Vogt and W. Grant, Jr., in A Study of the Phenomena of Apparent Movement, *Amer. J. Psychol.,* 38, 1927, 130-133.

shown. Neither of these disks rotates during the demonstration, but both are stationary before disk C, held there by the demonstrator with the open sector lying in any radius desired. The central holes cut out in A and B are just sufficiently large to allow them to slip loosely over the cap of the spindle on the motor, while the hole in C fits the spindle snugly and is screwed down to rotate with the motor. A and B are more easily held if they are somewhat larger than C. The two sectors in C are of white paper or white paint according to convenience. Upon the reverse side, C has a similar pair of red sectors.

Suppose one is demonstrating movement of the Müller-Lyer figure. The sector cut out of A will be turned to expose only this figure in B. With A and B held snugly against C no object will be seen, since A, B, and C are all black, unless a white sector on C lies behind one of the figures. Owing to the position of the two white sectors, both parts of the figure cannot be shown simultaneously; but they will be shown at a rate of succession equal to half the period of rotation of C. Disk C should be made to rotate sufficiently fast to produce apparent simultaneity of the two members of the Müller-Lyer figure at the outset. It is easy then, with the disks in position, to press the three disks together and slow down the speed of C, forcing the phenomenon of movement to pass from simultaneity through pure phi to succession.

Certain advantages of this demonstration are almost too obvious to mention. It is very simple to produce figures of various sizes and shapes, to see the movement taking place along vertical, diagonal or horizontal radii, and (by turning C over to expose the reverse side) to discover the relation of color of moving object to the filling out of the space over which the movement takes place either in the same color as the object or in some other color. The fact that the demonstration works without special illumination also enhances its practical usefulness.

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RECOVERY FROM VISUAL FATIGUE

The work reported in this note is a corollary of the problem done by Frank A. Geldard in this laboratory on the *Measurement of Retinal Fatigue to Achromatic Stimulation,* and reported in this Journal. The same apparatus was used and a description of it will be found in that article.

This problem deals with the recovery from fatigue and the relation which the intensity of the fatiguing light has to the speed of recovery. The terms "dark adaptation" and "recovery from visual fatigue" are taken in this paper to be synonymous, since both indicate a process taking place in the eye which results in increased sensitivity to light. Considerable work has been done concerning the rate of dark adaptation. Earlier investigators agreed that the fovea fatigued more slowly and recovered more slowly than the periphery. Hecht, however, doing the most recent work (1922) found that dark adaptation at the fovea proceeds very precipitately during the first few seconds, most occurs within 30 seconds, and the process practically ceases after 10 minutes.

The process of fatigue has received very little attention from psychol-

ogists and the recovery from fatigue even less. Von Kries is apparently the only person who has reported any figures on recovery. His apparatus for studying recovery was the same as that which he used for his work on fatigue, and is reported in the article mentioned. His results were as follows:—

TABLE I

Fatiguing time	Recovery time 0	5	10	20	40	80	160 (seconds)
160 seconds	9	17	32	50	71	83	100
16 "	50	70	70	80	87	—	—
40 "	35	39	53	66	85	—	—

The figures indicate the percentage of recovery reached after the given period; it is clear that in general the recovery is at first rapid and then slows down. (See the results for fatigue).

In the present experiment four observers were used, all of whom were trained to make a match within five seconds by being given a training period, which consisted of making 100 matches with unfatigued eyes. The latter procedure was as follows. The eye was fatigued to the left half of the circle for 60 seconds. At the end of this time the light was turned off and a recovery period given. The recovery times used were 5, 10, 20, 30, 45, 60, 90, and 120 seconds, and they were taken in irregular order. The whole circle was then lighted and *O* was asked to report the right, (i. e., the unfatigued half), in terms of the left. The eyes were used alternately.

In case the report was "brighter" or "less bright" the light was set in the correct direction on the next trial and *O* asked to report again. In this way a point was found at which *O* reported "equal"; this point did not vary more than a few centimeters on the scale at the outside, and in giving the results the average of these "equal" reports was used. The scale readings were translated into values expressing the seen brightness at various stages of recovery in terms of percentages of the "no fatigue" value. The results are so expressed for the three intensities used in Tables II, III, and IV. The intensities stood in the ratio of 1 to 4 to 100, and had the absolute values of 8224.5, 328.98, and 82.25 apparent candlemeters.

TABLE II

RESULTS FOR INTENSITY I

Recovery time	Ob. S.	Ob. P.	Ob. L.
0	5.6	8.6	22.4
5	7.6	25.8	82.8
10	14.5	32.7	92.9
20	20.09	47.1	100.0
30	33.1	61.8	——
45	40.09	71.0	
60	64.6	93.3	
90	76.2	98.9	
120	100.0	——	

TABLE III

RESULTS FOR INTENSITY II

Recovery time	Ob. S.	Ob. P.
0	23.5	35.9
5	25.3	——
10	28.4	38.6
20	35.9	51.9
30	42.8	70.06
45	61.9	77.2
60	72.9	100.0
90	97.6	
120	100.0	

TABLE IV

RESULTS FOR INTENSITY III

Recovery time	Ob. N.	Ob. L.
0	45.8	74.1
5	50.6	87.7
10	60.7	91.4
20	70.6	94.2
30	84.4	95.2
45	86.2	100.1
60	95.9	
90	100.0	

From a study of the results we find that in general the rise in the percentages, representing the relative amount of recovery from fatigue, is rapid at first and then decreases in rate. This is especially clear in the case of Observer L; here for intensity I the light drops to 22 percent of the original value, and in 5 seconds has risen to 82 percent. The results from S are quite different; here if the results are plotted in graphical form, they make a practically straight line, and the recovery takes much longer than for the other observers. The result from Observer N for intensity III show a decided similarity to those of Observer L for the same intensity, although the absolute values are different; L consistently shows a more rapid recovery than any other *O*. Observer P's results for intensity I resemble L's, though here again L recovers much more rapidly. For intensity II Observer P shows more rapid recovery than S, the original loss is much greater for S, and shows a much slower initial recovery. Although apparent differences between observers exist, they cannot be stressed, since the number of observations was too small. It is hoped that more work may be done on this problem in order that the conclusions may be substantiated on a larger number of observations. Unfortunately, owing to lack of time, it was impossible to run a complete series on each observer for each intensity; to do so would doubtless indicate more clearly the exact nature of the course of recovery; that there is a dependence of the rate of recovery upon the amount of fatigue induced is undeniable. As in the fatigue study it may be that a correlation exists between visual acuity and rate of recovery.

SUMMARY

Recovery is dependent both on the intensity of the fatiguing light (which determines the amount of fatigue induced), and the time allowed

for recovery. The experiment reported indicates this relationship. That the process of recovery is identical with dark adaptation has been shown; and the similarity of results showing the course of fatigue and those showing the course of recovery has been indicated. The need for further experimentation to establish more exactly the course of recovery has been stressed.

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BOOK REVIEWS

John Paul Nafe, Editor

Wilhelm Wundts Werk: ein Verzeichnis seiner sämmtlichen Schriften. Herausgegeben von Eleonore Wundt. Abhandlungen der sächs. staatl. Forschungsinstitute: Forschungsinstitut für Psychologie No. 28. München, C. H. Beck'sche Verlagsbuchhandlung. 1927. Pp. [iv] 78. Price Mk. 4.00.

Miss Wundt here gives us the complete bibliography of her father's writings. She speaks very kindly of the lists published since 1908 in the *American Journal of Psychology* by my collaborators and myself; and I am glad to know that they have been useful. Her own work, however, puts these earlier efforts out of the way. Wundt, for instance, began to review for the *Literarisches Zentralblatt* in 1871, wrote regularly till 1882, and did not end the connection until 1901,—with the result that there are some hundred and fifty reviews to be added to the bibliography; yet no one, without access to the editor's private list, could have assigned them to their author. Miss Wundt has brought together much more that is new to me: reviews, newspaper articles, various academic writings, and especially the publications of the war and post-war years.

The Bibliography has four parts. First comes the chronological list (1-33); Miss Wundt has not numbered the items under the years, which I think is a pity; the numbers are an aid to the eye, and may also be useful for quotation. Then comes an arrangement by subject-matter (34-58), very carefully done. Then follows a list of translations, by works and languages (59-66); used with caution, this list may be valuable. Finally we have a list of the lectures given by Wundt at Heidelberg, Zurich and Leipzig, from 1857 to 1917 (67-71). Psychology begins in 1862 as "Psychology from the Standpoint of Natural Science;" continues under that title till 1866-7 (except that in 1864-5 we have "Psychology as Natural Science"); and changes in 1867-8 to "Physiological Psychology," a title which appears for the second and last time in 1872-3.—Miss Wundt had added a general Index (72-78), which will be of particular help to those interested in Wundt's reviews.

The accuracy of the work can be tested only by use; where I have chanced to test it, it has proved correct. I have but two points of criticism. Miss Wundt does not say that the *Grundzüge der physiologischen Psychologie* was issued in two parts, pp. 1-464 in 1873 (*Lit. Zentralblatt,* 1874, 226), and the rest—as well as the whole book—in 1874; she gives the date of issue as 1874. Were this right, Brentano (whose preface, like Wundt's, is dated March, 1874) could not have had the first half of the work in his hands. The oversight is therefore important. My second point is, perhaps, arguable; I think, however, that the letter which Wundt wrote to Engelmann regarding the publication of the *Physiologische Psychologie,* since it has been published (Engelmann's *Jubiläums Katalog,* 1911), and since it is of great significance (*Amer. J. Psychol.,* 32, 1921, 596 f.), should have been mentioned,—if not as a special publication under 1872, at any rate in a footnote to 1874; certainly, no student of the *Physiologische Psychologie* can afford to neglect it.

Miss Wundt is to be congratulated on having brought her labor of love to a satisfactory completion; she has rendered a real service to psychology. Perhaps she will put us still more in her debt by further lists. I myself, years ago, started two other bibliographies; the one was a list of

the reviews of Wundt's books, and the other a list of books and articles and dissertations written about him. I gave up the first, because I could not in this country get access to many of the magazines I wanted; and I gave up the second, after collecting some thousand items, because I saw no end to the task. A bibliography of reviews would be useful.

E. B. Titchener

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Psychologies of 1925: Powell Lectures in Psychological Theory. By Madison Bentley, Knight Dunlap, Walter S. Hunter, Kurt Koffka, Wolfgang Köhler, William McDougall, Morton Prince, John B. Watson and Robert S. Woodworth. Preface by Carl Murchison. Worcester: Clark University, 1926. Pp. 412, 19 lectures. $6.00.

The book is divided among six groups of psychologists, there being four lectures on Behaviorism, represented by Watson and Hunter; one lecture on Dynamic Psychology, represented by Woodworth; three on Gestalt Psychologie, represented by Koffka and Köhler; five on Purposive Psychology, represented by Prince and McDougall; three on Reaction Psychology by Dunlap; and three on Psychologies called 'Structural' by Bentley.

For different reasons, such as newness of ideas or importance of subject matter, some of these lectures are outstanding. Hunter's polemic against the structural and configuration theories is carefully done and is clear and concise. But one feels that several of his points have been answered many times.

The suggestion that the name 'Anthroponomy' be substituted for 'Behaviorism' has much virtue, but for policy's sake it probably will not be acted upon.

His discussion is presented at a theoretical rather than experimental level because the problems to be discussed bear more upon the fundamental issues than upon matters of fact. His mention of names confuses one somewhat. The comments are evidently directed against the Structuralists except where he specifically mentions others but he cites, e. g., 'Wundt-Titchener-Bentley' as being one in the treatment of meaning.

He carries throughout a comparison of Anthroponomy and Psychology under the heads of (1) subject matter and goal; (2) method and (3) results. Taking these subjects in order he states that both schools take as their goal the understanding of some aspect of the human individual, leaving other aspects to other sciences, but this definition is narrowed a bit by inference. So, he states that the aim of both schools is to 'understand man' and 'study human nature,' (p. 85) definitions which could hardly be acceptable to either Wundt or Titchener.

He presents Titchener's differentiation between Physics and Psychology and rejects it as illogical and incorrect. Furthermore he finds that there is too much emphasis on the environment. Anthroponomy might be defined in similar terms: 'a study of the environment with man left in,' but would place the emphasis upon the man and not upon the environment. Because Psychology emphasizes environment it is an indirect *method of studying man.* Anthroponomy is a direct method.

In regard to method, Psychology employs two: individualistic and social (self introspection and reports from others) methods. He quotes Miss Calkins on the advantages of the individualistic method, and states that the only reason it has not been repudiated is that the data gathered

by this method form the basis for the interpretation of the results secured by the social method. This is an example of the rather confusing bringing in of names. If the views of Miss Calkins are offered as identical with Titchener's, I can see them both smile. Hunter gives four reasons why the social method is inadequate. On the other hand the anthroponomist, although he may experiment upon himself, does not use such results as the basis for interpreting results of his other work. Of course the method of Anthroponomy is the stimulus-response method.

In regard to the third problem, Hunter maintains that the results secured by Anthroponomy bear directly upon the nature of the human individual as he is found. The anthroponomist specializes more upon language behavior, learned responses, and the facts of inter-stimulation and response than any other scientist and in addition he cooperates with others in the study of various additional aspects of man in so far as these affect organic behavior.

The psychologist thinks that he secures two types of results: one he assumes concerns consciousness or experience, and the other, it is agreed, is behavior. But the behavior studies are very few and what few exist are so mixed up with experience that their value is doubtful.

The Gestalt psychologists differ from the Wundtians to some extent but are essentially the same so far as this argument goes.

Koffka offers an application of Gestalt Theorie to a problem especially important for Education but important also in psychological theory—the problem of mental development.

He posits three kinds of behavior: (1) reflex and instinctive; (2) trained or drilled performances; (3) intelligent achievement. He then reviews two types of current theories: the unitarian, deriving all behavior from one fundamental type, that of the reflex; and the pluralistic which maintains that there is an essential difference between the three types of learning.

The first type of theory depends upon a conception of activity as controlled by a system of fixed reflex arcs. But instinctive and even most of reflexive activity appears to be highly adjustive. He presents evidence to show that this is so, hence, he says, the theory breaks down at the reflex level but he pursues it much further.

To accept a theory of the second type, assuming that a new factor, the intellect, must be the cause, would on the one hand be equal to a renunciation of a theory of development, though he does not state just why this would be true, and on the other hand it would not even be an explanation of the facts.

Koffka then points out a third possibility: a situation forces a certain response, although the animal possesses no pre-established special devices for the act. He develops this thesis throughout the rest of the paper, relying, of course, upon the principles set forth under Gestalt Theorie.

Köhler, in his second lecture on Gestalt Psychologie, presents his general theory by applying it to the study of the facts of vision, where it has been worked out in greatest detail. The lecture is highly technical but logically arranged and clearly presented.

He examines the possibilities of sensation as the end product of psychological analysis as to whether this unit of analysis is conceived of as a differential, which is not a real unit, or as a unit comparable to a molecule of matter. This is an important matter for his theory.

He then examines the current conception of neural processes as to

whether they be conceived each as a separate system or if they more closely resemble, in principle of operation, a network of wires extending from one pole of a battery to the other. This is followed by examples of natural grouping and he gives the conditions under which some of this grouping occurs.

So, by analogies, examples and argument he brings us to his theory of physiological processes acting over a field which, when stimulated, or in any way changed, tends to a new state of equilibrium. Hence the perception of form or 'figure and ground' which, he maintains, is our only real psychological unit. Any analysis changes the experience and hence it is the ultimate for vision. The proofs offered and the objections met are too numerous to attempt to follow in a short review. His theory pictures a neural system as mechanical as that of the current theory but with less constraint. It works less like a light switch and more like a magnetic field where any influence effects changes in the whole system as it comes again to rest. His theory, if demonstrated to be true, would leave nothing of systems built upon such conceptions as that of reflex arcs and would, of course, greatly modify other systems. Structural systems would be most affected in their notions of extensity and their theories correlating sensory process and neural activity.

It is almost three years now since Köhler came to spend a year in America. It is surprising how little effect his general theory has as yet had. Evidently American Psychology is awaiting further proofs.

Köhler, in his first lecture, gives a report upon his work with chimpanzees. This work is too well known to justify a lengthy review. He takes occasion to rebuke the behaviorists for the extreme artificiality of some of their experimental conditions and for an almost negativistic attitude in their interpretations which he says is due to an exaggerated fear of criticism on the grounds of anthropomorphism. Generally he attempts to show that the learning of some of his apes resembles that of a human (child) more closely than it resembles the learning of lower animals. He draws tentative conclusions from these indications.

Dunlap's lectures are very easily read. The message is simply and clearly stated and the movement is rapid. After the rather technical articles previously reviewed, he is refreshing. With an easy tolerance for the mistakes of his earlier days and for the mistakes of others, he points out very precisely what those mistakes were and warns those who linger over these old ideas that they must catch up with the subject, which is moving forward rapidly, or presently they will be completely out of the picture.

In the first of his lectures he reviews the history of the theoretical aspects of psychology. Being a review it will hardly stand re-reviewing. His conclusion in regard to philosophical questions is that however interesting they may be to us as individuals, they have no place in our psychology.

The second lecture is on method and is comprehensive, though it may not be accepted, in toto, in all quarters. It also is in the nature of a review. The two lectures would do admirably for a text in orientation for graduate or advanced students.

His third lecture, on Social Psychology, is by far the most valuable in the book for persons with such interests. He asserts that in his opinion Social Psychology is now, for the first time, a possibility. The problems are now so formulated as to admit of psychological treatment and we now have a beginning by way of psychological facts which can be applied.

The social problems to which the application of psychological results, methods and principles is necessary he groups under four heads: (1) Problems of the sex life and family organization and functions; (2) Problems of religion and religious organization; (3) Problems of what may be briefly called civic and martial organization; (4) Problems of race and population. The particular questions involved are discussed at length.

The first of the chapters by Bentley is on the history of Structural Psychology. It appears that the term 'Structure' used in this sense originated from a note appended to an essay by William James in 1884 in which the idea of 'structure' was opposed to that of 'function.' The distinction was really made at an earlier period being inherent between the 'act' of Brentano and the 'observed process' of Wundt. Later Külpe and Ebbinghaus made use of the same distinction. Titchener accepted the term but with this difference: Although the others had made the distinction, they felt free to pass from one study to the other, but Titchener 'dug in', entrenched himself behind this partition and, as the author says, prepared to 'labor and give battle.' The distinction was greatly intensified after the rival claims of the structural and functional psychologies had been set forth by Titchener and James Angell. The great claim for Structuralism was its suitability for experimental attack upon psychological problems, and this is the criterion by which the author, in his second lecture, attempts to evaluate it.

We are now taken to some three or four reports of progress that occurred during the next twenty-six years. In the meantime Titchener had discarded the term 'Structural' as early as 1899, but the psychological system remained. Our author tells us that while this school, according to these reports, undoubtedly made important contributions, it is not true that they dominated the field for, while he does not again mention functional psychology by name, the inroads of practical interest, which is almost altogether functional in its psychology, became ever greater and it is not clear to him that the structuralists are really to be credited with the work done by the psychophysical metric methods, as good logic may force such works under the rubrics of function. It probably is impossible now to estimate the true influence of the movement, the development of psychology during the next several years only will show the part that Structural Psychology has really played during this earlier period.

Bentley's last chapter is on the 'psychological organism'. This organism is offered in contrast to the biological organism which is familiar to all. The biological point of view, our author states, has been productive within a fairly limited range of problems. But it must also be observed that many many facts of psychological import lie without this range though they are sometimes forced within it by a strained and illogical inclusion such as the attempts of the behaviorist to write 'objectively' of memory, imagination, thinking and the like, in terms of 'responsive' adjustments of the body to its environment.

It is this failure which suggests the distinction between the biological and the psychological organisms. This idea he believes will meet with resistance but it means nothing more than to psychologize those facts of life which include at once the experiences which we commonly call 'thinking,' 'wishing,' 'remembering,' and the like, and also certain zoological details of the body and its operations. The proposal lays emphasis neither upon a consciousness which inhabits or shadows the body nor upon a self-sufficient mechanism of flesh and blood. Such a conception

will also avoid bothersome philosophical conceptions, such as two 'aspects' instead of which we shall have one unitary organism.

He attempts to determine where this organism would stand: in relation to Behaviorism; the historical doctrine of Structuralism; configurationism; and psychoanalysis; and, toward the end, indicates the way in which he believes the conception of such a psychological organism contributes to the solution of the main problems of psychology.

The other lectures in the series are of the same general high class but most of them are too well known to need reviewing. Watson states the principles of his system, reviews his work on infants, pointing out the significance of the work in relation to theory, application and in regard to further effort along similar lines.

Woodworth follows with a presentation of Dynamic Psychology which, he says, is an attempt to characterize Psychology as essentially a study in dynamics, instead of allowing dynamics to occupy the relatively obscure position in the science that has so far been accorded it. The problem is to determine the 'workings of the mind' though futile questioning may be avoided by a substitution of the term 'organism' or 'individual' for 'mind.' It is concerned with antecedents and consequents, cause and effect, stimulus and response.

The study is not limited to emotion, conation and muscular and glandular action but also treats of sensations as dependent upon their stimuli, analysis of perceptions, the laws of learning, etc.

Morton Prince, in his first lecture, takes up three fundamental errors of the Behaviorists, devoting practically the lecture to this subject. He then turns to a proposed reconciliation of purposive with mechanical concepts. His second lecture deals with the evidence from abnormal psychology upon the questions of psychology, especially that of 'co-consciousness,' a term he offers as a substitute for 'subconscious,' and his third deals with the question of multiple personality by means of case histories.

William McDougall gives two lectures contending against the theory that men are mere mechanisms. He runs briefly through the history of Psychology, leading up to the modern conception of Behaviorism and the subsequent revolt of Watson and other Americans and the extremes to which they went. He recognizes three classes of Behaviorists on the basis of the extent to which they exclude purpose: The Watson group, which he calls the Strict Behaviorists, is characterized by their refusal to recognize both the facts of conscious activity and the fact that behavior cannot be adequately described, and still less intelligibly interpreted, without using language which recognizes the goal seeking nature of all behavior.

The second group is represented by Tolman, who refuses to be interested in only one of these groups, the facts of conscious activity. This group he nominates Purposive Behaviorists. A third group, represented by Allport, who have not definitely taken a stand on this question he calls the Near Behaviorists. He attempts to show the inadequacies of these systems but again, for lack of room, we cannot follow the argument through. Among other things he states that the Behaviorists of the strict variety, in their experiments, must give even rats a purpose and they do so by offering rewards and punishments. Hence their theory does not stand a pragmatic test. The Near Behaviorists fail when Allport substitutes the word 'drive' for 'purpose' which, McDougall maintains, shows that he cannot write his own results without using language implying purpose.

And Tolman, McDougall points out, is forced to use a whole vocabulary of words implying sensory experience, but deems it sufficient to add a note to the effect that the words are not used in their ordinary sense.

One notes throughout the series a general reaction against behavioristic theory that goes a bit deeper than mere disapproval, except, of course, from the behaviorists themselves. There is also some perhaps constructive criticisms of their methods. One is almost equally struck by the very infrequent allusions to psychoanalysis and the temper of the few references made. In spite of the rather large percentage of systematic discussion, there is much more evidence brought to bear than is often the case in such discussions—evidence that at least purports to be scientifically obtained. One gets a rather definite impression that the field is drawing closer together fundamentally rather than drifting further apart. Quite the opposite impression is received from current periodicals in regard to the work of psychologists which seems to be spreading almost too far for one science to hold. The similar series suggested for 1930 will be awaited with great interest.

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Conditioned Reflexes. By Ivan P. Pavlov. London: Oxford University Press, 1927. Pp. xv+430. 28 shillings.

Perhaps the most illuminating and broadly educative contribution—to the average Western scholar—of this profound and considered work will be its existence. Dr. Anrep, it seems, lectured at Cambridge; the imagination of the Royal Society was stirred; a grant for translation was forthcoming; and there unrolls before our still somewhat bewildered consciousnesses a record of painstaking, long-continued labor and intensive thinking and investigation rarely equalled in the countries conventionally supposed to be the normal habitat of "science." I quote a few organs from the fifteen-page bibliography of original published researches resumed here by the Academician: Bulletin of the Scientific Institute of Lesgaft; Proceedings of the Russian Medical Society in Petrograd; Russian Journal of Physiology; Archive of Biological Sciences; Bulletin of Military Medical Academy; Rusky Vrach; Kharkov Medical Journal. It has clearly become necessary to revise—and radically—our too provincial notions of West Europe and America as the special locale of disinterested investigation.

The *modus operandi* of the salivary reflex and the general technique of the work have been made more or less familiar in the West by the researches of Cannon, whose inquiries, however, postdated by some years the inception of the Russian work. Like Cannon, Pavlov and his colleagues are physiologists exclusively—or nearly so; very definite precautions are taken to maintain the argument completely outside the subjective sphere, although the existence of the latter (in man) is not denied. It is here—viz., in the philosophic groundwork—that the difficulties are greatest; they do not in any sense prejudice the argument—but they do engender questions which in the (visual analysers?) of the curious-minded will itch to be asked of the investigator. What exactly (disregarding for the moment the standard undergraduate prattle) *is* a reflex? It is (says Dr. Pavlov on page 4, quoting Descartes) a *necessary*

reaction to some external stimulus; it is determined—given the same stimulus, the same reaction *always* follows. The problem then is, given an underlying mechanist, particularist *a priori* formula for vital phenomena, required to analyze, experimentally, the observable phenomena in accordance with the formula; if units of the predetermined kind emerge, the formula is established. That they do, practically speaking, so emerge there can be little question; still, large sections of the book are devoted to demonstrating wherein the same stimulus (so far as is humanly possible to make it the same) does *not* give the same reaction. The answer, of course—if the preconceptions are to be preserved—is that it is not the same stimulus; it is not, in fact, the same dog, for he has had in the meantime half an hour's additional experience, so that the buzzer which before augmented his salivary secretion now inhibits it. Generalizing from this, it is never the same stimulus, and every individual act is a distinct reflex; wherein we have preserved the definition at the expense of its usefulness. No doubt all this makes little difference—only, what *is* a reflex? There is probably much to be said for the viewpoint of Dr. Cattell's friend who thus delimited psychology: "Oh, you know well enough what the mind is."

Not being under the physiologist's obligation to avoid trespassing on the subjective states, and similar unverifiable though attractive hypotheses, I found tremendously suggestive the concept of opposed excitor and inhibitor mechanisms, the aggregate balance between which is determined by heredity and determines temperament; and whose temporary vicissitudes are productive of such phenomena as irradiation and concentration of excitation, positive and negative induction, extinction and revival of reflexes, and—in extreme cases—genuine canine neuroses. The main features of the resemblance between the Russian reflexology and American behaviorism are sufficiently obvious, and indeed are mentioned in Lecture I; those between the former and Freud's psychoanalysis will bear investigation, though space prevents here. It is not more difficult to accept the inhibitor mechanism, for instance, than the repression; both are unconscious, both are conditioned, both are incapable of present neurological localization—and both engender neuroses when they come into violent conflict with the outgoing energies of the organism. In Vienna (and at Ithaca—the "investigatory reflex" is surprisingly reminiscent of "attention") a psychology has grown up; in Leningrad (and Watsonia) a physiology of the psychic organ. " . . . a small addition must be made to the wire at the central exchange." (C. R., p. 25.)

RAYMOND R. WILLOUGHBY

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An Introduction to the Theory of Perception. By Sir John Herbert Parsons. New York: The Macmillan Co., 1927. Pp. vi+254. $5.25.

Naturally one turns in a book by Parsons to the chapters on vision. In this case he finds a new theory and by Parsons himself.

Evidently influenced by Head's theory of protopathic and epicritic sensitivity of the skin, and the subsequent work of Head, Rivers, Parker and others tending to show a more or less general dual mechanism in the afferent nervous system, he has applied this theory to the facts of the

duplicity theory of vision. The review of facts is comprehensive but the presentation of the theory, of itself, is not convincing.

Besides this unified section there are several other chapters relating to the subject of vision and a long section which deals primarily with comparative anatomy, neurology, physiology, etc.

The balance of the book, which deals to some extent with very many psychological matters, is a most curious jumble of odd strays of philosophy, quotations and speculation which is not well arranged, the total effect being rather bewildering.

For those familiar with Parson's work on color vision, this book will be a great disappointment.

JOHN P. NAFE

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A First Laboratory Guide in Psychology. By Mary Collins and James Drever. New York: Dutton, 1927. Pp. viii+108. $1.60.

The outline of experiments included in this manual is based on the work done by beginning students in the George Combe Laboratory at Edinburgh. The plan of the book is not unlike that of the *Experimental Psychology* by the same authors. This manual is, in fact, supplementary to that volume and should find its greatest usefulness when used in conjunction with it. Fifty experiments are outlined; twenty of these are supplementary and optional. It is expected that the students will be able to perform only about twenty in the average beginning class in experimental psychology.

For the most part the plan of experiment is quite conventional; the problem is stated, the apparatus and materials are given, the procedure is outlined, and directions are given for treatment of results. There is lacking any discussion of the nature of the problem to be investigated such as one finds in such a laboratory manual as Titchener's. This volume scores in conciseness, however. For the instructor who seeks a laboratory guide, free from theoretical bias, this book should prove useful. For the most part the choice of experiments is excellent. Any defects that may exist as regards completeness are compensated for by the wide range of experiments offered. It will be found that demands upon special pieces of apparatus are lacking, a praiseworthy consideration in any outline designed to meet the needs of laboratories equipped only with the standard pieces.

FRANK A. GELDARD

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Principles of Abnormal Psychology. By Edmund S. Conklin. New York: Henry Holt & Co., 1927. Pp. x+457. $4.50; student edition $3.60.

This volume attempts a union of abnormal psychology and psychiatry. The first three chapters are devoted to a discussion of the principles of normal and abnormal psychology, and of such well-known psychological material as illusions, hallucinations, and memory abnormalities. This discussion gives the proper psychological background, and determines

that the remainder of the book shall have the atmosphere of psychology rather than that of psychiatry or the institution for the insane. The next four chapters of the book are devoted to the subject-matter usually found in a textbook of psychiatry that is not psychoanalytic. The classifications used are those of Kraepelin, well known through the translation of Diefendorf. There follows a chapter on shell-shock, which incorporates some of the material that has become available since the war. Following this there is a chapter on multiple personality, which incorporates the material of non-Freudian psychoanalysts. There follow two chapters devoted to the common subject-matter of psychoanalysis. There is then a chapter on hypnotism, followed by a chapter on spiritistic phenomena. There follow two chapters on sleep and dreams, and a chapter on the mental effects of drugs. Finally there is a chapter on feeble-mindedness and genius, and a concluding chapter on the historical and biographical background of abnormal psychology.

As is obvious, the book is much broader in its subject-matter than is any other book on this subject. It is intended as an elementary introduction to the entire field of abnormal psychology, and is the result of Professor Conklin's own teaching of the subject to his advanced undergraduates in the University of Oregon. Some readers may think that the presentation of the material is in too popular a form, but it probably contains as much nourishment as the average undergraduate will absorb. The book is wholly different in character from the specialized textbook in psychiatry, the specialized monograph on feeble-mindedness or on genius, or the specialized textbook on general dementia. The book will probably have a wide sale for use among undergraduates, but is not intended for use by graduate students. For the latter the specialized monographs are available.

CARL MURCHISON

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Social Psychology Interpreted. By Jesse W. Sprowls. Baltimore: Williams & Wilkins, 1927. Pp. xii+268. $4.00.

As the title implies, this volume is devoted entirely to the interpretation of the literature of social psychology, and does not attempt a serious consideration of the problems of social psychology apart from the formulations already expressed in the very extensive literature. Most of the divisions of the field of social psychology are treated in the fourteen chapters, but in all cases these various divisions are treated as being synonymous with their literary treatment. The author does not advance a new definition of the subject-matter of social psychology, but considers the various existing definitions, and attempts to indicate the general historical trend of such definitions. He presents an excellent analysis of the literature on instincts, social forces, and group mind theories. He finds that there has been very little agreement as to the nature of instincts, of social forces, or of group minds. Such a method is bound to create an intimate acquaintance with the literature on such problems, provided the student's interest is keen enough to keep him to the task. Most students are not interested in the problems merely as problems, and are likely to reach a condition of indifference if a certain amount of dramatic setting does not accompany a consideration of the problem.

The author rightfully asserts that there is at present no general agreement concerning the unit of social behavior, this unit being thought of by some as being an individual, and by others as being a group. The author suggests that this difference of view-point may result in two distinct sciences, social psychology and group psychology. He is convinced that behaviorism is not the key to social science, but that subjective, introspective methods must also be used. He feels, however, that social psychology is much more dynamic at the present time than it has ever been in the past. He attributes this change from the static to the dynamic point of view to the modern emphasis on individual variation.

If the reader is looking for a serious and careful analysis of the literature of social psychology, he can well afford to spend the necessary time reading this book. The book requires a certain amount of intellectual maturity in its subject-matter, however, and probably is not the best book for the immature student. Only the student with a very keen interest in the field of social psychology would continue to read after the first fifty pages. It is a good book to use as supplementary to a source book such as Kimball Young's "Source Book for Social Psychology." This would require a wholly different type of presentation from that required for the successful teaching of McDougall or of Allport.

The writing of such a book answers a very pressing professional need. It must be based, however, upon the assumption that there is some kind of coherency in the literature of social psychology. There are at least some who believe that such coherency is entirely absent.

CARL MURCHISON

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By Gilman & Co., Oxford, England

Edward Bradford Titchener

This photograph was taken just before Titchener left England for his student days at Leipzig, and is reproduced by permission of Sophia K. Titchener.

TITCHENER AT LEIPZIG*

FRANK ANGELL

In the fall of my last year at Leipzig, Titchener appeared in the general muster of the 'Institut' forces when Wundt was wont to give out a resumé of the investigations to be carried on during the coming semester and to assign the 'Versuchsthiere' to the several divisions. So far as outward appearances went, the new-comer from England was of a solid, sturdy build—often an index of considerable powers of endurance—with a shapely head suggestive of the way in which the endurance would be tested. He seemed quiet, and dignified with no trace of stiffness, and much alive to the exhibitions of learned naïveté not infrequent in the Leipzig Institut.[1]

Titchener's 'Arbeit' which gave him his doctorate was on the effects in one eye of binocular stimulation. At Leipzig he also worked up an investigation on time relations in an act of cognition. The latter takes up but seven pages in the "Philosophische Studien," but it was rich in the experience that in psychological observation many are called but few chosen. Of his nine observers Titchener reports he was able to use the judgments of but two.

In the general assignment of observers Titchener did not fall to my work nor I to his, but we were both billeted to Bruno Kämpfe's statistical investigation on the method of right and wrong cases. And here I began to get an insight into the essentials of Titchener's character. For of all the psychological work in which I have taken part, the 'reacting' in that investigation was the most tedious and, from faults in the apparatus, the most vexatious. Under these circumstances Titchener showed a spirit of loyalty and of responsibility for the outcome of the work which is not a too frequent quality of human 'Versuchsthiere.' In fact, whole-hearted participation of the observer in work of that kind implies unselfishness or devotion to scientific investigation; in his case probably both. The next year Titchener volunteered again for the resumption of Kämpfe's research, and it was owing in great part to what Kämpfe truly termed his "unermüdliche Ausdauer" that the work was brought to its statistical close.

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[1]My first meeting with Titchener was informal. I was clad in a long blue apron and smeared with sludge from cleaning my batteries when Titchener came in and, without introducing himself, inquired in halting German for Wundt. I gave him explicit directions in English. He told me afterwards it was for him an impressive interview. He had come to Germany with a high opinion of the acquirements of the Germans, but was nevertheless surprised at finding a laboratory 'Diener' who spoke fluent English without a trace of German accent.

Beside the grind of 'reacting' we played tennis together, foregathered much with Külpe, and, gradually passing beyond the stage of acquaintanceship, became friends. But no more than most of his fellow-countrymen was Titchener wont to wear his heart on his sleeve or give exhibitions of his powers, and it was only gradually that I came to surmise his depth of feeling and get a perspective of his range of attainment, though I early became aware of the difference in training between Balliol of Oxford and a small New England college—especially in the Classics.

About the middle of the fall semester Titchener suggested to me that we work together on a translation of the third edition of Wundt's "Physiologische Psychologie." In the course of thirty-seven years I have forgotten the details of plan of work, but as he had brought over from Oxford a complete translation of the third edition I dare say the project had to do with a revision. Titchener thought that either the Pitt or the Clarendon Press might publish the translation, but neither Oxford nor Cambridge showed any eagerness to undertake the publication. The correspondence with both was long-drawn out, and in the meantime Wundt had laid down the fourth edition on the ways, so the plan of collaboration, whatever it might have been, was dropped.[2]

The strongest impression I took away from our undertaking was of Titchener's remarkable power of turning out work. John Morley in his "Recollections" relates that Clifford, the mathematician, wrote the essay on the "Unseen Universe," taking up twenty-eight pages of the *Fortnightly*, in a single sitting, lasting from ten o'clock at night to nine of the following morning. I could credit Titchener with a like ability. In his admirable memorial to Titchener in the *American Journal of Psychology*, Boring speaks of the enormous amount of work involved in the preparation of the four "Manuals." More specifically I would refer to the "Introduction" of some one hundred and seventy pages in the instructors' "Quantitative Manual." It is a critical history of Weber's Law centered on Fechner's methods and theories, and it involved the hardest and closest reading to be found in experimental psychology of all the psychophysical investigations published during a period of forty-one years. It would be an exhausting task for anyone to do in the time Titchener gave to it, and a hopeless task for anyone without Titchener's Macaulay-like power of racing through printed matter.[3]

[2]In the "Translator's Preface" to the fifth edition of Wundt's "Grundzüge," of which the physiological part appeared in English, Titchener gives a brief account of his translations of successive editions of the work. It is a comprehensive picture of his 'unermüdliche Ausdauer.'

[3]Huey had cited a case of a man who read a novel of 320 pages in two and one-fourth hours. Titchener did not think this exceptional; "I certainly read at the same or at a higher speed"!

In his first lecture on the "Experimental Psychology of the Thought Process" Titchener criticizes the English Associationists as not distinguishing theory of knowledge from the theory of thought. In talking over the characteristics of the new members of the Institut with Külpe I found he thought Titchener was strongly of the same bent. I think I agreed with him and I still have the impression that for some time Titchener was wont to substitute logic for psychical data in discussion. But in the course of time he came to free himself so thoroughly from the bonds of 'Logicizmus' as to criticize Wundt for occasional lapses into the "rationalization of experience" in the "Physiological Psychology."

In serious discussion Titchener spoke with a quickness which was faster than the usual rapid-fire rate of the educated Englishman and his statements of fact and arguments were delivered without much parliamentary padding. The personality of a speaker or an opponent in discussion was for him not at all in question; he was simply trying to form a correct opinion, and his attitude towards others in debate was as if he were addressing mere vocalized intelligence, or at any rate something wholly impersonal. This was the attitude he looked for from others, and while welcoming opposition he was impatient of deprecatory interludes.

To Külpe, until he came to understand Titchener, the latter's quick, incisive manner seemed brusque and to some it may have given actual offense, especially to anyone with a congested amour prôpre or a ticklish self-conceit. For pettiness of spirit Titchener had a contempt which he made no effort to conceal, though exhibitions of naïve vanity, not unknown in a large group of adolescent savants, merely moved him to kindly laughter. He himself was above all forms of pettiness and, while he had no lack of self-respect, that quality did not swell into self-importance. He really enjoyed the 'roughing' of his traits, national and personal, which was dealt out to him from time to time.

In his dislike of sentimentality as well as in his distaste for any fervid expression of feeling, Titchener ran true to English form. We were speaking of Walter Pater, who occupied a minor niche in Titchener's Pantheon as well as in mine, when I delivered a monologue, tinged, I fear, with gush, on the famous purple patch with which Pater had indued Mona Lisa Gioconda. "She is as old as the rocks among which she sits, etc." Titchener heard me to the end and then merely remarked staccato, "Oh yes! Very fine! Rocks and eastern rugs and all that sort of thing." Naturally the German contingent in the Institut were slow to comprehend the sensitiveness and depth of feeling which lay behind Titchener's manner, but in the end they became friendly—Meumann, Külpe, and Kirschmann

were close friends. From outside the Institut, von Frey came to be more or less associated with this group.

Fellowship with Titchener was a staff of comfort to me in my last year at Leipzig, and I said good-bye to him with regret—strong regret. I liked his fine and generous spirit, his contempt for shams, and I admired his ability and breadth of culture. Next to Külpe, I thought he was the ablest of the younger generation of psychologists, and as such commended him to the academic authorities at Cornell. It is many years since I last saw him. We kept up a desultory correspondence—half professional, half personal—usually confidential. As a psychologist he went beyond my anticipations formed in Leipzig days; in steadfastness as a friend he met my expectations.

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THE PROBLEM OF THE CONDITION OF AROUSAL OF THE PURE SENSATION OF CUTANEOUS PAIN*

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In spite of the extensive treatment which it has received for some time, the problem of pain remains today still unsolved. On the one side, theories are still struggling for recognition which see in pain only a feeling of unpleasantness of high degree (e. g., an additional quality arising by way of a central excitation-process) or, as in all the so-called sensory feelings, an "affective sensation" of specific quality. On the other side, too, there is as yet no agreement among those investigators who view pain in its pure form, not as a "feeling" or as an "effective sensation," but as a well-localized elementary sensory content of specific nature, which is accompanied by a more or less intense feeling of unpleasantness. Since I can consider here only a view of this sort, I, at the same time, reject those views which find in pain a mental experience standing between feelings and sensations.

It is obvious then that when the simple experience of pain is accepted as a characteristic sensory content, the question of the condition of its arousal must come to the fore. This is the question around which the conflict now rages with particular violence, and to whose solution this paper offers a modest contribution.

Among those who have studied this question, there have developed two opposing tendencies, which are connected with the names of A. Goldscheider and M. von Frey. The difference between them goes so deep that a reconciliation is hardly conceivable. On the contrary, as things stand, if one view is correct, the other must necessarily be false. Consequently, we cannot expect the conflict to end until one of the views attains general recognition and so vanquishes the other. Since the results of my own work have forced me on the side of von Frey, I need scarcely mention that my report upon the condition of arousal of the pure sensation of cutaneous pain can speak only in favor of von Frey's theory of pain.

Both views agree that pain is to be assigned to the sensory realm. While Goldscheider classes it with the "common feelings,"[1] he is far from regarding it as a "feeling" or an "affective tone."[2] Gold-

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[1]*Zsch. f. Sinnesphysiol.*, 57, 1925, 1ff., 31.

[2]Das Schmerzproblem, 1920, 65.

scheider's whole treatment of the problem of pain rests on the conviction that pain is a "specific sensation," which may "not be identified simply with the affective tone, unpleasantness."[3] To be sure, Goldscheider would have been more consistent if, instead of speaking of "common feelings," he had accepted the concept of "common sensation" introduced by W. Wundt.[4] Since the distinction between "feeling" and "sensation" is psychologically imperative and is recognized by Goldscheider, it is incomprehensible why he has not carried through the distinction in his treatment of *cœnesthesis*. Had he done so, his whole exposition could only have gained in precision. But apart from this, Goldscheider's definition of the sensation of pain is unmistakable.

Another point of agreement between the views mentioned is that, for both, the sensation of pain depends upon the functioning of a special class of nerves. Goldscheider writes: "In the realm of the sensations of sight, hearing, taste, smell, and temperature, there is no pain. The dazzling pain, the painful sensation with shrill tones, the pain of cold and of heat, etc., are mediated by the co-excitation of certain sensory nerves for pain and not by the specific sensory nerves themselves."[5] The same view might easily be illustrated further from other places in Goldscheider's writings.

From this point on the ways separate. Whereas von Frey tries to produce more and more new proofs of what has already been made highly probable by O. Funcke,[6] namely, that the sensation of pain is bound up with the functioning of specific nervous structures which have nothing to do with the production of other sensory qualities, Goldscheider denies outright the existence of specific pain-nerves. Referring to the well-known observations of M. Schiff, Goldscheider states that the sensation of pain arises through the mediation of general tactile nerve fibers which run through the gray substance of the spinal cord and that the conditions of the arousal of pain should be sought there. These conditions Goldscheider sees in a summation of subliminal pain-excitations. He also believes that this assumption is in agreement with the observations on the irradiation of the sensations of pain.[7] At the same time, he grants that pain-excitations can break through the gray substance even without a process of summation.

What led Goldscheider to his theory of summation was the phenomenon of the secondary sensation of pain, which he and Gad investigated together. This secondary sensation of pain appears readily upon

[3]Das Schmerzproblem, 1920, 35.
[4]Grundzüge der physiologische Psychologie, II,[8] 1910, 47.
[5]Das Schmerzproblem, 1.
[6]Hermann's Handbuch der Physiologie, III, 1880, 2, 296ff.
[7]Das Schmerzproblem, 66ff.

sudden contact of the skin with a pointed object, as well as upon stimulation of the skin by break induction shocks that are under the pain-limen.[8] Goldscheider believes that this secondary sensation can be explained only by means of his theory of summation. But while he has to grant that the secondary sensation can also be underneath the pain-limen, he refers it even in this case to a summation of excitations occurring in the gray matter of the spinal cord.[9]

To indicate my own position as over against Goldscheider, I should mention first that I believe that his notion of the nature of pain contains a contradiction which, so far as I can see, cannot be eliminated. Goldscheider, with his doctrine of pain, still stands in the position of Joh. Müller, who assumed that the sensation of pain arises "by way of the violence of the feeling-excitation."[10] Pain, for Goldscheider, is a higher degree of intensity of the tactile sensation. It is subsumed under the concept of tactile sensation,[11] and, indeed, in the same sense as Joh. Müller subordinated it (together with sensations of warmth, cold, voluptuousness, etc.) to the concept of the "feeling-sense" which was current at the time.[12] On the other hand, Goldscheider maintains that the sensation of pain is of specific character. By all usage, this expression can only mean that the sensation of pain is in no way comparable with the sensation of touch, which unquestionably is also a quite specific mental experience. Goldscheider, it is true, tries to qualify the meaning of the expression by stating that while the quality of the "superficial" sensation of pain is "specific," yet it "resembles the other sensations of the sensory nervous mechanism which is adjusted to mechanical stimuli."[13] Although by such twists the contradiction may be partially veiled, it cannot be eliminated. One may characterize the individual sensations of light, smell, and taste—in short, the elementary mental phenomena belonging to any one qualitative group as similar to each other; but that will not do in comparing the sensation of pain with the sensation of touch. The two sensory qualities are, psychologically regarded, as different from each other as, for example, the odor of a flower is different from its color. Sensations of touch and pain can, naturally, combine into a mental complex in which one of the two components may stand out more than the other; but in themselves they are specifically different from each other. The distinctions here involved are of the sort which Helmholtz[14] tried to characterize by means of the concept of

[8] *Gesammelte Abhandlungen*, I, 1898, 44, 397. Concerning the delay of the sensation of pain upon mechanical stimulation of the skin, *cf.* O. Funcke, 299f.
[9] Das Schmerzproblem, 67.
[10] Handbuch der Physiologie des Menschen, II, 1840, 498.
[11] *Cf.* Das Schmerzproblem, 83.
[12] *Op. cit.*, 249.
[13] Das Schmerzproblem, 65.
[14] Die Tatsachen in der Wahrnehmung, 1879, 8.

modality. For the psychological view of the matter, there can be no doubt at all about this point. In his first investigations, Goldscheider too realized this difference, although at the same time he already considered pain to be "but a modification of the sensation of the feeling-sense."[15] And if today he wishes in a certain sense to moderate the distinctions by assuming a similarity between the two sensations, then it seems to me to prove that, after all, he himself recognizes the contradiction in his view of the sensation of pain and is attempting to resolve it. The reconciliation, however, is impossible. The sensation of pain shows no resemblance to the sensation of touch; it is a sensation of peculiar quality, comparable with no other.

Goldscheider's view of pain as an intensified sensation of touch leads to further difficulties. If it were a fact, then, as Goldscheider himself has repeatedly admitted, it would stand alone in the whole doctrine of sensation. In no other field of sensation can a case be found in which an analogous situation occurs. Everywhere the intensive increase of a sensory quality with increase of stimulus follows, in general, the uniformity expressed in Weber's Law, until a point is reached above which no further increase is noticeable. This point has been named the "terminal stimulus." Even in Ewald Hering's ingenious doctrine of the sensations of light and color, the opposed sensations are connected with specific nervous processes, the antagonistic processes. A process of assimilation or dissimilation, no matter how much it may be intensified, can never convert its corresponding sensation into a new quality. In this case, too, the perceptibility of an increase in intensity finds its limit at the terminal stimulus. This uniformity is likewise verified in the realm of the sensations of touch; it also holds for the sensation of pain. And so it is everywhere. In so far as we are in a position univocally to control and to maintain the external form of energy, in none of the fields of sensation known to us does the impression aroused by the external stimulus change into a new quality when the stimulus is strengthened by a certain amount. How can we conceive that in the development of the tactile mechanism, a mechanism which is so important for all forms of animal life, an exception in psychophysical evolution should have occurred such that, in the same nervous mechanism and depending upon the amount of energy acting upon it, processes should be released leading to two basically different sensations? If, in accordance with the doctrine of specific energy (which is better called the doctrine of the specific functioning of nervous mechanisms), we must assume that specific nervous processes underlie characteristic sensory differences, then in the present case we must presuppose that one and the same form of energy, depending upon its degree of intensity,

[15] *Ges. Abhandl.*, I, 43.

undergoes in the tactile nerve-apparatus two specifically different transformations, such that their intensive changes stand to the intensive changes of the sensory qualities connected with them in a relation which in each case conforms to the requirements of the fundamental psychophysical law. As we have already mentioned, however, there is no analogy in any of the fields of sensation which have up to now been closely investigated. For this reason, Goldscheider's view of the origin of the sensation of pain contains an improbability which makes his doctrine, in the absence of irrefutable proofs, appear unacceptable.

And how are we to conceive the summation process which Goldscheider posits? In the light of past experience, it is an impossibility. Experience teaches that no new sensory quality can ever be produced by the summation of simple mental processes of like quality, or of the nervous excitations which condition them. Something really new appears in the mental realm when qualitatively different psychical processes combine into a complex. This, however, never happens by way of a process of simple summation but, on the contrary, by a creative principle which, for that very reason, Wilhelm Wundt called the "principle of creative synthesis." So far, no enduring objection has been brought against this principle. It governs the whole of mental life, and it is verified in the combination of the simplest experiences as well as in the formation of the most complex mental content that we know of. In view of these facts, the Goldscheider doctrine of the origin of the sensation of pain arouses considerable doubt also when considered from the purely psychological point of view.

It should be realized, further, that in the case of the nervous processes which condition sensations of pain, we are dealing with the function of a protective mechanism which is of the greatest significance for the preservation of the organism. This Goldscheider too has eloquently depicted.[16] One can, indeed, unhesitatingly add that the pain-mechanism (in addition to other mechanisms) even represents a weapon without whose aid the development of the animal organism in its varied forms would not have been possible. But if this is so, then it should not seem too rash to assume also that the formation of this peculiar conscious content, this "warner" (Goldscheider), is connected with the functioning of quite special nervous mechanisms. The presumption is confirmed, at the outset, by our experience that just those parts of the body which need an especially high protection also possess the greatest sensitivity to pain. Consider, in this respect, parts of the body like the cornea and the conjunctiva of the eye-ball. And this presumption would grow to certainty with the experimental proof that in the central part of the cornea only painful sensations can be aroused; that the conjunctiva is sensitive to pain, but not to

[16] Das Schmerzproblem, 82.

touch; and that the conjunctiva and the margin of the cornea, though sensitive to cold, are not sensitive to warm. Exceptions have been found only in the case of the *caruncula lacrimalis* and for the outer rim of the *conjunctiva palpebralis*.[17]

Together with Brückner, Goldscheider has undertaken a test of von Frey's statements of 1894. The experiments were performed on the cornea before and after the cocaining. The investigators obtained the following results:

Before cocaining: Mixed touch-pain sensations; tactile sensations bordering on pain; pure sensations of pain, which however are specified as "rare"; pure sensations of contact, or pressure, which usually, but not always, were unpleasantly toned. At the same time, there were isolated cases in which no sort of sensation appeared as a result of stimulation.

After cocaining: Pure sensations of contact and of pain, the latter of which are designated again as "rare"; "sensation of contact with unpleasant feeling-tone; sensation of contact with a doubtful sensation of pain; sensation of contact with a suggestion of pain; sensation of contact with pain following; pain with a diffuse sensation of contact."[18]

Goldscheider admits, in his discussion of these results, that they do not yet prove the presence of a unitary nervous mechanism for the mediation of sensations of touch and of pain. But since he lays great stress on the fact that pure sensations of contact and of pain are observed but rarely in these experiments, he considers it as ultimately "most probable" that in the cornea we have to do with a unitary nervous mechanism.

In order to reach convictions of my own in this question, I have recently repeated the experiments on my eyes. The experiments were performed with small stimulus-rods, about 6 cm. long. On the pointed end of a rod, I twisted a little flake of sterilized cotton that ran out into a fine point. I kept on hand a number of such small, easily made stimuli, and with them I performed during several successive days 100 separate experiments on each eye. The eyes were not cocained. In these experiments I use the eyes alternately and always worked with dry cotton.

The results of these experiments agree with Goldscheider's reports in the respect that, in isolated cases, no sort of sensory point appeared on the cornea. More often, insensitive points were found on the *conjunctiva bulbi*. But even with respect to the various regions of the cornea there appeared a difference, since I was unable to find insensitive points in the central region of the cornea. Every contact

[17]M. v. Frey, *Leipziger Berichte*, **46**, 1894, 192. v. Frey u. Webels, *Zsch. f. Biol.*, **74**, 1922, 173. Strughold u. Karbe, *Zsch. f. Biol.*, **83**, 1925, 199.

[18]A. Goldscheider, Das Schmerzproblem, 37f.

with the cotton point was there sensed as painful. Only about the margin did a few insensitive points appear. For the rest, my observations differ from Goldscheider's reports. I note, first, that it was impossible for me to evoke *pure* sensations of touch on the cornea, although I was keenly on the lookout for them. And the same is true of the conjunctiva. Every actually sensed contact was, in both places, more or less painful or toned with pain. I was able to observe differences in intensity, but not a single *pure* sensation of touch. In so far as I was able, with my apparatus, to check up in detail the differences in intensity of painfulness, I found that the central parts of both corneas were most painful. The sensitivity to pain was so great there that I was not able to suppress the reflex when these places were stimulated. I can, furthermore, confirm the fact, first observed by E. Marx, that the sensitivity of the cornea to pain is greater in the direction of the horizontal diameter than it is in the direction of the vertical.[19] To make sure of this fact, I had to proceed gradually, with the stimulus-rods, only from the edge of the cornea toward the center in both directions. It is also of interest that the painful sensation on the conjunctiva often takes on a coloring somewhat different from what it is on the cornea. On the conjunctiva, it is also less narrowly circumscribed than it is on the cornea. The peculiar coloring of the painful impression on the conjunctiva I can account for in no other way than as a blend of a rather intense sensation of pain with a weaker, inadequately aroused sensation of cold. As Goldscheider found, and as one can easily determine through proper stimulation of the skin, isolated cold points are analgetic. The sensory complex with which we are dealing here can, therefore, be regarded as having arisen in no other way than through simultaneous stimulation of a pain spot and a cold spot. In view of the density of the cold spots which Strughold and Karbe found on the lateral parts of the conjunctiva,[20] this need not be surprising. I may add that, with a somewhat firmer cotton point, I was able several times to evoke inadequately an almost pure sensation of cold on the temporal side of the conjunctiva of my right eye.

I should also point out that after the appearance of the painful impression, either on the *cornea* or on the *conjunctiva bulbi*, a sensation of itch appeared, as was to be expected. Even when the first impression on the side parts of the cornea and on the conjunctiva was not so intense as to lead to the reflex, the sensation of itch was, at times, so unpleasant that I had to close the eye and rub the outer side of the eye-lid with my finger. I may add, that, on the cornea and the conjunctiva, I was never able to arouse a pure sensation of tickle, which, in my opinion, depends upon the tactile mechanism.

[19]Strughold und Karbe, *loc. cit.*, 191.
[20]*Ibid.*, 192.

Besides on the edges of the eye-lids, I was able to arouse the sensation of tickle only on the *caruncula lacrimalis* where, even with weak stimulation, the sensation appeared with fair intensity. Alexander, whose observations are reported by Goldscheider,[21] says that he had a "feeling of tickle" on the cornea. If he did not touch an eyelash, then a sensation of itch, which is not always strictly differentiated from the sensation of tickle, is really involved. The observer suffers from an old *blepharitis squamosa* and, consequently, often has to remove carefully, with pincers, tiny scales and hairs which have fallen on the cornea. In doing this, he asserts, according to Goldscheider, that he senses "at first only contact (along with a feeling of tickle)," and "pain only with stronger pressure." As for the expression "contact" (*Berührung*) which the observer employs, it is much too loose to give any definite indication of the sensory quality actually involved. On the basis of my own observations, I presume that the "contact" mentioned was the initial stage, or a weaker degree, of the sensation of pain, which was then followed by a sensation of itch. The initial stage in the course of a sensation of pain can, in fact, be easily confused with a tactile impression; but the observer who is practised in sensory analysis can have no doubt of the qualitative difference between the two experiences. The pain has, from the very beginning, an affective tone that is altogether different from the tone which accompanies any stage of the sensation of touch. From the beginning it takes the direction of unpleasantness. The sensation of pain, depending on its locality and the physical process which evokes it, may run its course more slowly or more rapidly, so that the initial stage is of longer or shorter duration. Indeed, in certain circumstances, it may even remain in the initial stage and fail to rise to higher degrees of intensity altogether; but, in every case, its quality is different from that of any stage of the sensation of touch. Let one but observe how a toothache makes its appearance and how it takes its course. In this course, the initial stage of the sensation of pain can often be observed very well. A comparison of it with the pure sensation of touch will always show, however, that the two experiences are qualitatively different.

The same is true of the terminal stage of a sensation of pain. If one carefully follows up the terminal stage of a sensation of pain which is slowly dying away to complete disappearance and compares the separate phases of it with cutaneous sensations of touch, one is sure, I believe, to become convinced that in this case quite specific differences are involved throughout.

Just as, on the one hand, we observe that organs which, like the eye, require considerable protection are also extraordinarily sensitive to pain, so, on the other hand, we are impressed by the fact that sensi-

[21] Das Schmerzproblem, 38.

tivity to pain is reduced or is completely lacking in those parts of the body which can do without this protection. Thus, I made the determination that the mucous membrane of the cheeks as well as the back parts of the mouth, including the back half of the tongue, are but slightly sensitive to pain; that the lower half of the uvula is completely insensitive to pain stimuli; and that there is a place in about the middle of the mucous membrane of the cheeks, which with suitable mechanical and electrical stimulation (the latter was first used by von Frey) is sensitive to touch but not to pain.[22] These facts have been confirmed so often that their correctness cannot be doubted. Likewise, the distribution of touch and of pain spots, and accordingly the sensitivity to touch and to pain stimuli, is not the same in the different regions of the body. If we bring all these findings and those previously mentioned into relation with certain anatomically demonstrable facts and with certain other psychophysical facts, as well as with certain pathological observations, then we must, I believe, come to the conviction that Goldscheider's hypothesis of a unitary nervous mechanism for the mediation of sensations of touch and of pain is not tenable, and that, instead, separate mechanisms for these sensations are to be assumed.

On the basis of anatomical findings, it has been demonstrated that the Meissner corpuscles and the nerve-skeins of the hair follicles are organs of touch. Of this there can be no doubt. Likewise, there is nothing to contradict the assumption that the nerves ending in the epidermis mediate pain. The two kinds of nervous structures are found at considerably different depths, and in the hairy regions of the skin, the nerves ending in the epidermis occur in incomparably greater numbers than do the others. To the organs of touch there correspond the touch spots; to the nerves ending in the epidermis, the pain spots of the skin. Strughold,[23] in a model investigation of the density and the limens of the pain spots, has recently shown that their average density on the hairy skin amounts to about 175 per sq. cm., while for the areas of active touch only an average of 54 per sq. cm. could be found. This density is altogether different from that which I determined for the touch spots a number of years ago.[24] A further interesting finding of Strughold is that at the tip of the nose the touch spots predominate and the density of the pain spots is surprisingly small. These facts should not be especially difficult to understand if they are considered from the genetic standpoint.

With respect to other psychophysical observations, it may be recalled that cutaneous sensations of touch and pain show entirely

[22] *Arch. f. d. ges. Psychol.*, **55**, 1926, 37; *ibid.*, **56**, 1926, 452.
[23] Würzburger Inaugural-Dissertation, 1924.
[24] *Philosophische Studien*, **19**, 1902, 260.

different reaction-times and that their localization on the skin is not the same. I also take the liberty of pointing out that, in the stovain experiments performed some years ago by Dr. M. Ponzo[25] in my laboratory, there appeared this difference in the effect of the drug: sensitivity to pain seemed to return to the *full* norm sooner than tactual sensitivity. Results that are freer of objection may be obtained by the methods especially perfected by Rein,[26] those of electro-osmotic deadening (*electrosmotische Vertaubung*). Schriever,[27] in his investigations of the sensitivity of the oral cavity to pain, applied this procedure to a circular area, 4.2 cm. in diameter, on the volar side of the forearm. He found that tactile sensitivity disappeared with the second degree of *Vertaubung*, while the area still responded with pain when stimulated by a pointed bristle with a force of 8 gr. Further experiments of this kind upon different areas on the skin should yield more information concerning the difference in behavior of the two sensory mechanisms.

With respect to the pathological observations mentioned above, it should be mentioned that Funcke[28] has already pointed out the great difference in sensitivity to touch and to pain which occurs at certain stages of ether and chloroform narcosis. My own experience confirms this observation. I may remark further that years ago I investigated a hysterical young man whose skin was fully sensitive to touch, but who did not feel the least pain from pin-pricks, cuts, and strong electrical stimuli which no normally sensitive person could have endured.

A few years ago, von Frey[29] described a new method which makes it possible to produce on the skin undisputably *pure* sensations of pain, i. e., sensations of pain not blended with tactile impressions or other cutaneous sensations. For this purpose, he used the spines of *cardus acanthoides*, fastened to the free ends of bristles whose stimulus value had been determined beforehand on the chemical balance. Von Frey then reported experiments with five persons, which were performed upon the skin, over the distal end of the *musc. biceps brach.*, by means of "spine bristles" 0.5 gr. and 1 gr. in stimulus value. In 250 tests, 57.6% pure sensations of pain were reported for the 0.5 gr. stimulus and 41.6% for the 1 gr. stimulus, while in 16%, 6.4%, and 20% of the tests with the 0.5 gr. stimulus and in 4%, 10.8%, and 43.6% of the tests with the 1 gr. stimulus, there was observed no sensation, "only contact," or "contact and pain," respectively. The method is excellent, and also signifies a great

[25] *Arch. f. d. ges. Psychol.*, **14**, 1909, 425.
[26] *Zsch. f. Biol.*, **81**, 1924, 125.
[27] *Ibid.*, **83**, 1925, 431.
[28] *Loc. cit.*, 297. *Cf.* the further statements of Funke.
[29] *Zsch. f. Biol.*, **76**, 1922, 1.

step forward since it should remove all doubt that the sensations aroused at the touch spots and those aroused by stimulation of the pain spots are mediated by entirely different intervening processes within the skin. The other results can be looked up in von Frey's article; I have tested them all and have been able to verify them fully.

For an entirely different purpose, I have recently performed 100 experiments on each of a number of parts of the body, 9 cm. square in area, by means of a new æsthesiometer which I have already described elsewhere,[30] and with spines of *opuntia robusta*, 2 gr. in stimulus value. The results are summarized in the following table. I was the subject of these experiments, and one of my assistants was the experimenter. As in von Frey's experiments, no touch and pain spots were fixed beforehand; stimulation was haphazard; but it did not go beyond the boundaries of the area specified. The subject kept his eyes closed while being tested; his judgments were immediately recorded by the experimenter. The values given in the table represent percentages. The hair was shaved off.

TABLE I

	No Sensation	Contact Only	Sensation of Touch Only	Contact and Pain	Sensation of Touch and Pain	Pain Only	
Hairless Region of Left Forearm 13/4/27; 5 P. M.	0	0	26(+2?)	2	32(+3?)	35	=100
Middle of Volar Side of Left Forearm 16/4/27; 5 P. M.	3	9	24	8	11(+7?)	38	=100
Left Elbow-crook 10/5/27; 5 P. M.	9	7	24	16	13(+3?)	28	=100
Middle of Volar Side of Left Upper Arm 24/4/27; 5 P. M.	3	21	27	3	19	27	=100
Left Knee-cap 6/5/27; 3:30 P. M.	0	6	25	10	19(+15?)	25	=100
Middle of Front Side of Left Upper Thigh 4/5/27; 4.30 P. M.	3	10	28	11	15(+3?)	30	=100

In the table, a distinction has been made between "contact" (*Berührung*) and "sensation of touch" (*Tastempfindung*). Fre-

[30] *Arch. f. d. ges. Psychol.*, 57, 1926, 396; *Arch. ital. di psicol.*, 5, 1927, 4.

quently no distinction is made between the two terms. In my opinion, however, a strict analysis of cutaneous sensations requires that the term contact be given a univocal meaning if it is not to lead to mistakes; for every cutaneous sensation can, in fact, be designated as contact. The contact can be sensed as warm or cold, as painful, or as a simple tactile impression. Following the example of von Frey, I designate as a sensation of contact a fleeting, diffuse sensation of touch, such as one can easily obtain by applying a stimulus that lies at or near the limen to a touch spot, or a stronger stimulus to a point close to a touch spot. The true sensation of touch, on the contrary, always has a granular, intermittent character. When this characteristic appears especially strongly and distinctly as, for example, on the skin of the elbow-crook, the tactile impression that comes from the stimulation of a single touch spot may be sensed, at times, as a slight tickle, an experience which is to be distinguished from the sensation of itch. Under certain conditions, of course, both impressions may blend into a total experience.

From what has been said it is already clear that the "sensation of contact" as well as the "pure sensation of touch" can enter into combination with the sensation of pain. Naturally, the perceptible interval between the sensation of contact and the appearance of the sensation of pain is longer in the former case than it is in the latter.

The table shows a considerable number (indicated in the table by means of heavier print) of sensations of *pure* touch and of *pure* pain in every one of the tested areas. If the two experiences are examined carefully, the great qualitative difference between them is easily recognized; at bottom they are incomparable. Whereas the sensation of *pure* touch retains the intermittent character mentioned above, the qualitatively different sensation of *pure* pain presents an unbroken continuum; it resembles a narrowly circumscribed insect-bite, and, in its last stage, it turns into an experience which is designated as a sensation of itch and which usually has a greater spread. The sensation of itch, which often lasts a long time, may also appear alone when a stimulus which is not too strong happens to hit a point on the skin that is not a pain spot but lies near one. Such a point, naturally, may also be a touch spot.

If the pain spots are fixed beforehand and then stimulated several times in succession, it will be noticed that the receptor organs corresponding to them fatigue easily. The organ of touch, to be sure, fatigues also, but, according to my observations, not nearly so easily nor to as great a degree as the single peripheral organ of pain. When, in a procedure without knowledge, pain spots which had been determined beforehand by means of a lens and then fixed, were stimulated even but once, or twice in succession, with a stimulus value of 2 gr., they often broke down completely with the following application of

the stimulus, and a long time passed before they were again able to function.

All of these observations, in my opinion, lead to only one conclusion: *that the condition of arousal of a superficial sensation of pain must be different from that of the sensation of touch; that no unitary nervous mechanism can be assumed for the two sensory qualities; but that, instead, the possibility of their arousal is to be sought in the existence of separate nervous mechanisms.*

It is a question of great interest whether the temperature-pain is conditioned by neural organs other than those which condition the superficial pain evoked by purely mechanical stimuli. Rollet has already, though in a very cautious way, made such a conjecture for the warmth-pain. More recently, Rein[31] has tried to raise this presumption to certainty by means of extraordinarily careful experiments. Without deciding the question of what are the true receptors of the stimulus for warmth-pain, he observed, on applying cocaine-cataphoresis, that when a cutaneous area, 3 sq. cm., was stimulated, after superficial pain and sensations of touch, cold, and warmth had faded out completely, with temperatures of 40° to 47°, there still appeared a deep sensation which he designated as pure "burn." The author concluded that the arousal of this warmth-pain must be attributed to end-organs which lie still deeper than the end-organs of the sensation of warmth. In so far as my experiments on the oral cavity, by means of which I tried to test Rein's results, enable me to form a tentative judgment in the matter, I believe that these results cannot be denied at least a high degree of probability. It is certain that superficial pain and Rein's warmth-pain differ in sensory content. It is to be noted that the oral cavity is, in general, but slightly sensitive to warmth, whereas the warmth-pain, in my experience, appears there in an intensive degree. Should Rein's results be indisputably confirmed by further experiments, the theory of summation would scarcely be in a position to explain the phenomenon, whereas the view that in this case too there are involved nervous mechanisms of specific nature which have been formed in the course of phylogenetic development presents no difficulty in the way of understanding the phenomenon.

Concerning the arousal of cold-pain there is as yet no definite evidence like that which Rein has produced for warmth-pain.

Of late the question has several times been discussed whether there are "deep sensations of pressure,"—a view which von Frey[32] and his collaborators, Rein and Strughold, have definitely rejected. I beg to point out that many controversies might be avoided if the term "pressure" (*Druck*) were applied exclusively to the mechanical

[31] *Zsch. f. Biol.*, 81, 1924, 202ff.
[32] *Ibid.*, 82, 1925, 359.

stimulus, and if instead of speaking of "pressure spots" and "sensations of pressure," one spoke of "touch spots" (*Tastpunkten*) and "sensations of touch," in the same sense as one speaks correctly of "pain spots" and of "sensations of pain," or of cold and warm spots and of sensations of cold and warmth. Even the mechanical pain stimulus shows a pressure value; hence, if the sensory content is not chosen as the basis for the terms in question, one can, in principle, designate even pain spots and sensations of pain as pressure spots and sensations of pressure. But if we speak of "touch spots" and "sensations of touch," as we have done in this paper, instead of speaking of "pressure spots" and "sensations of pressure," then the question of the existence or non-existence of deep "sensations of pressure" cannot even be raised; for the specific sensation of touch always remains, as we know, bound up with the functioning of specific organs of touch. Also, the concept of sense which has come down to us from the older psychology still continues to produce confusion in the present-day doctrine of sensation. So long as we do not make up our minds to avoid this unhappy and, at bottom, metaphysical concept, empirical sensory analysis will, as I have repeatedly pointed out, remain burdened with a load that can but hinder its progress.

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TROPISMS*

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I

The well-known monograph by Loeb (1918) summarized his position with regard to the chief evidence then available concerning tropistic conduct and the deductions which might follow from its analysis. It also served to indicate certain important steps still to be taken. Loeb's approach to the problem of individual organic behavior was initially and primarily philosophic, in the sense that through recognition of the elements of action and the modes of their control he looked to find an answer to the puzzles of free will and determinism. With the insight permitted to a superbly penetrating intelligence, he was able to see beyond the immediate consequences of relatively simple experiments and to extract from them their utmost general significance. A quantitative treatment of tropistic behavior is essential if the understanding of conduct is to be furthered in this way. It is curious and noteworthy, however, that very seldom do current general discussions of the tropism situation appear to realize the significance of the quantitative aspects of behavior. By "quantitative" one does not wish to imply simply that which has to do either with numbers of observations or of animals (there are weird and lovely instances of even this in the literature!), nor yet merely numerical data which may be submitted to statisticoid jugglery. One *does* indicate, rather, numerical data which may be submitted to analysis in terms of intelligible theory; the "quantitative" has significance in the light of functional interrelationships which the analysis may reveal, and of the tests which it suggests.

From this standpoint the recognition and the characterization of a particular tropism in a specified organism is scarcely an exciting end in its own right. The tropism in question may become, however, a powerful instrument of analysis under suitable conditions. If it can be shown, for example, that in controlled circumstances the direction of movement of given animals may be predictably determined even when more than one orienting force is at work, we then have at hand a means (as yet in an elementary stage of development) whereby a second-order test of theories of oriented movement may be applied. Moreover, as I hope to indicate, this general procedure may possibly be employed to obtain information about central nervous states and conditions, not otherwise approachable. This is in addition to the

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fact that modes of tropistic response adequately expressible in reasonably simple formulæ may be used to investigate peripheral sensory processes as well. It is proposed to summarize in outline a group of interrelated inquiries designed to bear upon these topics.

II

If the tropistic behavior of animals is in any real measure that of simple machines, it should be possible to deal with such problems as the following: knowing the orienting efficiency of light of a particular composition, from a single source, it should be possible to predict the form and amount of the orientation when two sources play upon the animal. The question is but deceptively simple, yet it can be answered. It involves quantitative knowledge of the connection between light intensity and orienting efficiency, and it necessitates assumptions (which can be tested) regarding the central nervous interplay between excitations of different peripheral origins.

It is desirable to be specific in discussing this matter, because one detects the prevalence of two general bodies of opinion regarding tropisms. If I may somewhat harshly overstate the case, one type of opinion seems to regard the whole matter as settled; and the other, as being of no moment. It would be difficult to determine which view falls wider of the mark. Even in the comprehension of the problem, so far as this may be revealed in definitions explicit or implied, one constantly encounters curiosities. For example, the conception implied in some recent comments by Overstreet (1925).

Scarcely more strange is the notion, which I have also encountered, that "a tropism is a definite response of an animal with a nervous system." The latter, as a definition, is obviously meaningless, though Professor Overstreet's remarks present a larger gallery of positive misstatements of fact—some five of them indeed. A tropism is most effectively defined as an oriented movement in an energy field. When the field is a luminous field, part of the desired information may be had from circus movements. In the slug *Limax maximus* the eyes are the effective photoreceptors. With illumination from above, a dark-adapted *Limax* from which one eye has been removed circles continuously toward the non-stimulated side. The circling posture is continuously maintained until photic adaptation supervenes. The posture of the body is due to differences in the maintained tensions of the muscles of the two sides of the body wall. The tension difference is maintained only under illumination, and disappears in darkness. With proper care to insure absence of air currents, odorous stimulation, and the like, and with proper selection of phototropic individuals, a measure of the orienting posture is easily secured. The experiment is particularly significant because the muscular mechanisms for creeping and for turning are struc-

turally distinct. It is necessary, however, to take account of the fact that the two are nervously interrelated in such a way that faster progression (controlled by the release of pedal waves) involves less effective turning. The two mechanisms can be separated in an analytical way through their relations to temperature (Crozier and Federighi, 1924-25) and in other ways. A large number of such experiments has been made by Cole and Crozier, in which the circling trails could be accurately recorded and measured. With comparable speeds of progression, the phototropic effect is measured by the ratio *degrees turned per centimeter of path.* While it might seem to be a matter of simple common sense, it is nevertheless necessary to emphasize that for such experiments selection of individual animals is just as necessary as control of light intensity and of temperature. A confusion unquestionably exists in some quarters, as to the objective of such experiments; it seems to be supposed that the purpose is to examine a random lot of individuals of a particular sort, and to note whether they are phototropic. This kind of inquiry results, at best, in statements to the effect that under such-and-such conditions *x per cent* of the organisms were seen to respond; which tells us precisely nothing about the character or the measurable attributes of the response when it is exhibited. At the moment we are concerned to discover the relation between light intensity and orienting effect in phototropic *Limax,* and non-reactive individuals (such as are easily produced at will by experimental treatment; Crozier and Libby, 1924-25) are of no interest. It turns out that the circus movement effect, expressed as the amount of turning per unit length of path, is directly proportional to the logarithm of the light intensity, over a fairly wide range. This kind of relation is often labelled "Weber's Law," apparently with the thought that it is thereby explained, and even in cases where explicit caution against this has been clearly stated (*cf.* Crozier and Pincus, 1926-27, *b*). The status of the Weber-Fechner formulation in relation to photic excitation has been adequately treated by Hecht (1919-25), and need not detain us. What is important is to realize that such empirical relationships may be extremely useful, where formulations of greater theoretic implication are clumsy or unmanageable. There are other methods whereby the orienting power of light from a single source may be expressed (*cf.* Wolf and Crozier, 1927-28; Crozier and Wolf, 1927-28; Yagi, 1927-28), but their explanation involves considerations not yet introduced in this treatment. In general, however, they lead to two forms of relation, in different cases: the orienting power is directly proportional to the light intensity, or, more commonly, to its logarithm. Both relations are consistent with elementary photochemistry, and with the fact that for the production of a given (measured) photosensory effect with light of a given intensity the Roscoe-Bunsen law

(or Talbot's law) is adequately obeyed (*cf.* Loeb, 1918; Hecht, 1919-20, *a*; Northrop and Loeb, 1922-23).

III

When circus movements effects are studied under proper conditions (*cf.* Cole, 1922-23; Crozier and Federighi, 1924-25; and the preceding paragraph) the illumination does not change during orientation, and the resulting data should permit one to predict the outcome when a phototropic organism is illuminated from one side. This case is sometimes complicated, however, by the kind of effect which has led to v. Buddenbrock's (1917) conception of *Lichtkompassbewegung*, namely that, since a positively phototropic caterpillar may approach a small source of light in its own plane along a *spiral* path, it is really responding in such a way as to put its body axis at a constant angle to the light-rays reaching it from moment to moment. This behavior has been carefully examined by Crozier and Stier (1927-28, *b*) in larvae of *Malacasoma* (tent-caterpillars). A path such as v. Buddenbrock requires would be a logarithmic spiral, which has well-known simple properties. The actual paths do not exhibit these properties. Therefore the *Lichtkompass* effect need not be invoked in such cases, and indeed is no explanation. The spiral paths are in fact due to the simultaneous exhibition of two separately identifiable modes of response: the orienting effect of the light increases the tonus of longitudinal muscles on the illuminated side, and the bending of the caterpillar's body so produced, evokes the body-straightening movements which constitute the homostrophic reflex (Morgulis, 1910; Moore, 1922-23, 1923-24; Crozier and Moore, 1922-23; Crozier, 1923-24). The animal consequently proceeds under the influence of two directive vectors—one, increasingly preponderant with closer approach to the light, directed to the light and varying as the inverse square of the distance (or its logarithm); the other, leading by itself to tangential motion with constant speed, is expressible as of constant magnitude. The actual path represents a continuous adjustment between these two forces, and trails of the kind found are easily obtained by graphical construction on the assumption that they are the efficient controlling elements. It is of interest, none the less, that the equations of such loci, for all the simplicity of the assumptions, are practically impossible to obtain. But this is purely a matter of mathematical complexity, and cannot fairly be charged to the unpredictability of the conduct of the caterpillars. There is illustrated, however, one kind of reason, often overlooked, why analytical experimentation must involve rigidly simplified conditions; it is true in this field, as in others, as Boscovitch long ago suspected, that it is not so much a question of the complexity of Nature as of the deficiency of our mathematics.

IV

The quantitative prediction of photic orientation under conditions which obviate the kind of mechanical interference just described can, however, be carried out successfully (*cf.* Crozier, 1926-28; Mitchell and Crozier, 1927-28). Consider a negatively phototropic animal on a horizontal surface illuminated by 2 point-sources of light. The X-axis of a system of reference coördinates passes through the lights, the Y-axis bisects the distance between them. The animal at any moment has its anterior end A at a point (x, y), and the coördinates of the lights L' and L'' are respectively $(-a,o)$ and (a,o). Then the distances AL' and AL'' are $m^2 = (x+a)^2+y^2$ and $n^2 = (x-a)^2+y^2$. The dimensions of the photoreceptors are safely assumed small in relation to m and n. The lights L' and L'' have luminous intensities P' and P'' candles respectively. Orientation should be accomplished and maintained when the photochemical effects on the two photoreceptive surfaces are equal. This is the *only* assumption of the tropism doctrine, and it turns out to be sufficient. Taking first the case in which the organism is attached to the substratum, but able to orient, the position of orientation is defined by the positive angle θ which its axis makes with the X-axis connecting the lights, and it may be shown that the necessary relation between θ and the other quantities concerned is:

$$\tan\theta = \frac{P'n^3\,[\tan h(a+x)+y] + P''\,m^3[\tan h(a-x)+y]}{P'n^3[a+x-y\tan h] - P''m^3\,[a-x-y\tan h]},$$

where h = the angle between a photoreceptive surface and the longitudinal axis of the body. For an ideal organism which is *positively* phototropic similar equations are obtainable which differ only in detail. The formula given, and simpler expressions of it easily deduced for various cases, hold equally well whether the primary photosensory effect is proportional to the intensity of the light, I, or to log I. With negatively phototropic organisms illuminated by beams of parallel rays opposed at 180°, the equation reduces to

$$\tan\theta_1 = \tan h\,\frac{I'+I''}{I'-I''};$$

with beams crossing at an angle of 90° (parallel rays),

$$\tan\theta_2 = \frac{(I'+I'')\quad(1+\tan h)}{(I'-I'')\quad(1-\tan h)};$$

(*cf.* Crozier, 1926-28, where these equations are directly deduced,

but with $\theta = 90° - \theta_1$ for the first case, $\theta = \theta_2 - 45°$ for the second). When the light rays are parallel, there is no necessity for restricting forward movement in testing the equations.

These equations can be tested by a number of series of measurements. In some forms, as the larva of the blow-fly, the angle h is constant. With other forms it is zero, and in such forms the fact that $h = 0$ can be directly checked by failure to obtain orientation with lights opposed at 180°. A further check is given by the known facts of orientation (*e. g.*, in experiments due to Buder, 1917-19) for forms in which $h = 0$, namely various protista swimming in a helical ("spiral") path, and in which beams of light cross at an oblique angle. In this case, from the general formula one obtains

$$\tan \Phi = \frac{\sin \Psi}{I'/I'' + \cos \Psi},$$ where Φ is the angle of the path of orientation with the axis of one of the two beams, and Ψ is the angle between the beams. The agreement with experiment is extremely good.

The angle h is not constant, however, in all cases. In the blow-fly larva the sideward swings of the anterior end are connected with the mechanism of progression, and their frequency and extent are independent of illumination. With certain other animals this is not so. If photokinetic excitation results in "random" movements of the head, the frequency, speed, and amplitude of such movements should influence the calculated value of h, and this should be a function of the total intensity of excitation; h would be expected to decrease in proportion either to $(I_1 + I_2)$ or to $\log I_1 I_2$. In young rats (before the eye-lids have parted) this is its behavior (Crozier, 1926-28; Crozier and Pincus, 1926-27, *c*). In other forms h increases with low total intensities of illumination, due to the suppression of random motions resulting from other kinds of excitation, and then passes through a maximum as the highest intensities provoke increasing "random" movements of the head. It may be noted that there is obtainable in this way a *measure* of "random" movements, and a key to their interpretation.

When these considerations are applied to the case of a *moving* animal in a field illuminated by two point-sources of light in the same horizontal plane, further difficulties arise. To distinguish this situation from that already treated we may speak of orientation of a *sessile* form as presenting a problem of orientation *in situ*; the orientation of a motile form in beams of parallel rays is of course dynamically identical with this; the phototropic progression of a motile organism may be termed orientation *in transitu* (*cf.* Mitchell and Crozier, 1927-28). Here, the path of the animal from any point

in the illuminated field depends upon the direction in which it is moving when subjected to the lights at that point. Differential photochemical effects in the two photoreceptors affect the rate at which the slope of the path varies with time (or with respect to distance covered, if the velocity of creeping be sensibly constant). The change of slope is $d\theta/dt$, but we cannot at once decide whether in such an instance the rate of turning depends upon the *difference* between the excitations on the two sides, or upon their *ratio*; and in either case there may be a direct or a logarithmic relation between excitation and the luminous flux. If the rate of creeping is very small, we have (from the general formula already given):

$$\frac{dy}{dx} = \frac{P'n^3\,[(a+x)\tan h+y] + P''m^3\,[(a-x)\tan h+y]}{P'n^3\,[a+x-y\tan h] - P''m^3\,[a-x-y\tan h]}.$$

Since m and n are complicated functions of x, y and a, the expression is practically unsolvable as a differential equation, although it can be tested by several modes of approximation.

The elementary conditions of oriented movement have been reviewed in order to bring out the basis for a particular conclusion. Regarded simply as the orientation of a machine with bilaterally disposed photoreceptors (the symmetry may be dynamical, as in helical-swimming protista, rather than merely of an obvious structural sort), the description of phototropic movement is seen to present a very fair degree of complexity. This becomes apparent at once if we seek to predict the character and extent of orientation when more than one source of light is acting, on the basis of what is known concerning phototropism in the simplest kinds of situations. Inability to deal quantitatively with the measurable aspects of the orientation of organisms in compound fields of illumination, therefore, is not to be taken gratuitously as evidence either of incomplete understanding of the case, nor, on the other hand, as indicating supra-mechanical influence at work within the organism. In an elementary way the kinds of complexity revealed by the present analysis serve to demonstrate concretely the justification for quantitative investigation under the most rigidly simplified conditions attainable, to which testable formulations can be applied.

It is not always true that the simplest conditions will be found in the most obvious place. An especially clear instance is found among galvanotropic phenomena. In the case of annelids (Moore, 1922-23) and other metazoans, the interpretation of galvanotropic orientation turns upon the necessary and sufficient assumption that the current serves to excite definite groups of nerve cell bodies. This turns out to be consistent with the mode of action of strychnine, which pro-

duces a relatively specific reversal of inhibition in responses to peripheral excitation, and also reverses the mode of galvanotropic response. These effects seem to have considerable generality (Crozier, 1926-27, 1927-28; Fries, 1927-28). It is historically and in other ways of interest that galvanotropic movements were early reported as occurring in the roots of plants, and subsequently examined in various animals. It turns out (Navez, 1926-27) that the "galvanotropic" curvatures of roots, which because of their slowness and the definite manner of their exhibition might be useful for quantitative investigation of the mechanism of stimulation in a structurally simple system, are, however, artifacts due to chemical products of polarization at the electrodes ordinarily used. The "galvanotropism" disappears if the electrodes are made non-polarizable.

The point here raised is intimately connected with another. It is sometimes voiced as a reproach that simplification requires "unnatural conditions"—whatever "unnatural" may be supposed to mean. An instance is supplied by the phototropic circus movements of *Limax*. Experiments over a range of temperature show that in the performance of circus movements under light of constant intensity two chief factors are involved—the rate of creeping, and the amplitude of turning. The structural basis for each is distinct, but the two are functionally interrelated in various ways. Within a zone of temperatures in the immediate neighborhood of 16°—15° C., which probably would be commonly accepted as "normal" for this animal, the response is distinctly more variable, quantitatively, than at temperatures either higher or lower. Above 16° the amount of turning increases with rise of temperature, while below 15° the reverse is true. In this region of "normal" temperature conditions, the balance between (1) the tendency to release pedal waves, which result in progression, and (2) the turning tendency, is close to a critical point (for numerous related phenomena, *cf.* Crozier, 1924-25; 1925-26). Slight circumstances may determine in particular instances which of the two effects is dominant. Hence, statistical variation in the amplitude of turning per unit length of path. At higher temperatures the turning mechanism is in control, and the slug creeps only so fast as the central mechanism having to do with differential bilateral tonus will permit. At lower temperatures the amount of turning is limited and controlled by the speed of creeping, to which it is inversely proportional. The "control" of creeping by the central nervous determination of differential tonus in the musculature of the body is demonstrated by the results of treatment with strychnine (Crozier and Federighi, 1924-26, *b*) which suppresses phototropism.

Under "natural" conditions, consequently, certain kinds of variability in response may be fairly conceived to result from the fact

of delicate dynamic adjustment among processes which (in effect) compete for control of the animal's mechanisms of movement. The perhaps curious principle ensues that, for real understanding of the determination of conduct and the identification of its elements, it is practically essential to place the organism under distinctly "abnormal" conditions such as are favorable to the unhampered expression of separate elements of the usual nexus. From a slightly different standpoint the behavior of *Onchidium* is illuminating in this respect (Crozier and Arey, 1919; Arey and Crozier, 1921).

When this is done in the examination of phototropism it is found possible to account for orientation under the influence of two beams of light, on the basis of a single assumption derived from experience with a single source. The fact that quantitative theory can be tested by experiment only under certain simple conditions is due not to a failure of the analysis, but to mathematical intricacies attending the orientation of a non-living machine. The problem of orientation with *three* sources of light is for example practically unsolvable, merely as an exercise in mathematics. It is fair to point out, and necessary to do so, that the execution of the pertinent experiments requires recognition of variables which are often disregarded by those who most delight in pointing to the complexity of organic behavior. In fact, it is legitimate to say that the kind of vagueness often apparent in reports of investigations in this field seems to result not infrequently from failure to conceive clearly the possible end of the analysis in view.

Even when in a given compound field of illumination it is possible to account precisely for the amount of orientation exhibited by a phototropic organism, certain constants remain in the equations. It is tempting to regard these as in part "threshold" constants, and as such perhaps relating to the mechanism of central nervous adjustment between competing excitations. In an organism exposed to two sources of stimulation the resultant movements may be determined by the summation or by the balance of the respective excitations; with other suitable conditions the movement may be decided by one of the two excitations to the complete exclusion of the effects of the other. Such a "decision" may be taken to represent an elementary act of central nervous adjustment, and, given rational units in which to express the effects of the two sources of excitation when acting singly, its nature may be investigated by measuring its quantitative modification under controlled conditions. There should be procurable, in this way, a means of approach to central nervous functions which might hope to effect a rationalization of their nature (*cf.* Crozier, 1924; 1926-28; Crozier and Pincus, 1926-27, *d*; Crozier and Stier, 1927-28, *b*). In this respect the quantitative description

of tropistic orientation can become the kind of analytic tool which I have earlier hinted at.

The primary difficulty in such studies is of course the essential requirement that the integrity of the organism shall not be violated. The very thing which we desire to learn about depends upon the absence of destructive interference; we must not employ such treatments as may provoke irreversible changes. There remain, free from such objection, certain additional definite avenues of approach. Taken singly and in combination, we have a right to expect that these may assist in the way desired. There is first the modification of the temperature of the organism, and the consideration of the relations between temperature and the frequencies or velocities of measurable activities (*cf.* Crozier, 1923-28). It happens that these relations "make sense" in terms of elementary physical theory, and the ways in which they can be modified experimentally (*cf.* Crozier and Stier, (1925-26) already yield suggestive and significant conclusions (*cf.* Crozier, 1923; 1924, *b*; Crozier and Federighi, 1923; Hoagland, 1927). The processes of excitation by light (Hecht, 1918-28) permit similar treatment. The methods of genetics, combined with such procedures, make possible to a high degree the unification of the experimental objects as uniform and fairly comparable individuals, and a few cases (where enough is known of the genetic behavior) even permits the making-up, at will, of individuals possessing particular characteristics required for specific purposes. Examples of this will be mentioned subsequently. At the moment I desire merely to stress the importance of "analysis without interference," and to consider illustrations of its use.

V

The proposed treatment requires that there be available antecedent acquaintance with individual modes of tropistic behavior. The compounding of tropistic modes of behavior has been relatively little studied in any exact way, partly because the necessary preliminary descriptions have not been available. We may lay a basis for the interpretation by considering the case of geotropic orientation. The method is not restricted to this case, however. The balancing of phototropic and stereotropic excitations, for instances in which it is possible to show that the stereotropic effect obeys the laws of compounding already indicated for phototropism (*cf.* Crozier and Moore, 1922-23; Crozier, 1923-24; 1926-27; Crozier and Pincus, 1926-27 *a*), provides one illustration. A more recent one illustrates an additional feature. By compounding the effects of galvanic and photic fields mutually perpendicular, it is possible (Crozier and Stier, 1927-28 *a*) to find for each of several intensities of light a current density which forces orientation of marine planarians at exactly 45°

to the axis of either field. The galvanic excitation must then be equal, in its tonus effect, to the photic. Since the latter is practically proportional to log I, we expect the current density (for 45° orientation) to be a rectilinear function of log I. This turns out to be true. Several conclusions follow. The organism behaves, in this case, not as if the compound situation involves properties peculiarly its own, but as it should if responding to each kind of excitation separately; the effect is purely additive. Moreover, one secures a measure of the galvanic excitation in energy units even without knowing how much current goes through the organism; and since the excitation is found proportional to the current density one has direct proof that the current probably stimulates by moving ions—as is usually assumed, but without demonstration.

A positively geotropic animal, such as a tent-caterpillar, orients upward upon an inclined plane, but, in the absence of interfering modes of stimulation (*i. e.*, under non-stimulating weak red light), it does not creep "straight upward" unless the surface upon which it moves is nearly vertical. The upward orientation ceases at an angle θ with the intersection between the creeping plane and the horizontal. At various inclinations of the surface (α) it is found that the average value of θ (*or* the geometric mean) is a definite function of α, such that, purely as an empirical relationship which has definite useful properties, $\theta = K \log \sin \alpha$.

If we seek to obtain the reason for this effect, the conditions of progression must be examined with care (*cf.* Crozier and Stier, 1927-28, *b*). When this is done it is noticed that the anterior end of the caterpillar swings from side to side during locomotion. The possibility of a control of the angle θ is therefore given if we assume that orientation ceases when the difference between the pull of the weight of the anterior end of the body (free from the substratum at the execution of each lateral swing) in the "up" position and in the "down" is reduced to a threshold magnitude. Since the active component of gravity, in the plane of movement, is obviously proportional to $\sin \alpha$, this assumption is expressed by the equation $K \sin \alpha \, [\cos (\theta - h) - \cos (\theta + h')] = const.$, where h and h' are the mean angles between the axis of the anterior swung portion of the body with the main axis, in the "up" and the "down" positions respectively. There are means of showing that h' and h are statistically equal. The equation at once reduces to

$$(\sin \alpha)\ (\sin h)\ (\sin \theta) = K'.$$

If h be sensibly constant, and independent of θ (in the average),

$$(\sin \alpha)\ (\sin \theta) = K''.$$

If α be constant, θ must vary inversely with h. By means of trails

upon smoked paper, permitting measurement of the lateral excursions of the head, the latter assumption can be tested, and is easily verified (Crozier and Stier, 1927-28, *b*). When a large number of trails are studied (about 3,000) this variable cancels out, and we should find sin θ inversely proportional to sin α. This expectation is thoroughly realized in the experimental results. The case is of interest in several ways. The "head angle" is similar to that previously involved in the discussion of phototropism, and is essentially a dynamic thing rather than directly a matter of structural symmetry. These facts are to be compared with the geotropic orientation of such an organism as the slug *Agriolimax* (Wolf, 1926-27), where a *structural* angle of the required sort is readily perceptible. In this case also the formula just given is definitely obeyed, as demonstrated in Wolf's paper.

It is to be recognized that such formulae describe *positions* of orientation, and only indirectly have to do with questions of the *intensity* of stimulation. This should be sufficient in itself to prevent one from regarding the logarithmic formulation first given as an example of the Weber-Fechner rule. While it is impossible to convert the logarithmic formula into the second, trigonometric one, as a matter simply of mathematics, the former retains a definite utility in connections in which the trigonometric expression is unmanageable. Moreover, as I shall later indicate, it is possible to obtain some insight into its real meaning.

The first experiments looking toward this type of formulation were made with young mammals (Crozier and Pincus, 1926-27, *a*; 1926-27, *e*; 1926; Pincus, 1926-27; Crozier and Oxnard, 1927-28; Keeler, 1927-28). Such experiments with rats and mice require special attention to the state of the test-animals, their care and handling, and to the conditions of the tests. Since the results are susceptible to a particular kind of mathematical treatment it is important to realize that the results of this treatment can be checked in several independent ways. Hunter (1927) has objected that this work has not been described with adequate detail, and claims that the analysis of such data is faulty. His paper, however, contains internal evidence that some of the presumed "difficulty" is due to lack of acquaintance with elementary mathematics, and a larger part to so hasty a perusal of the articles in question as to give the (perhaps unfair) appearance of a deliberate ignoring of a number of essential points specifically treated. (For example, Hunter states that we have found the geotropic orientation to obey "Weber's law," although we gave specific reasons (Crozier and Pincus, 1926-27, *b*) for contrary opinion; also that "a part of the data" have been given in the graphical form used to demonstrate the effective significance of the relation $\theta = K$ log sin α; this statement is simply untrue; I

mention the matter merely as indicating ground for insistence upon more careful reading of the deliberately compact accounts previously given; the insistence is justified by a number of additional occurrences in this particular paper.)

The point of the experiment consists in determining the relationship between the angle θ, already defined, and the inclination of the surface (α). To do this individual litters are selected from lines of rats long inbred by brother-sister or other close mating. This insures genetic uniformity of individuals, and can be demonstrated to be a necessary precaution. Small litters are chosen, so that all the rats may be vigorous creepers; this also is necessary. At the age of 12 to 14 days, before the eyes have opened, such individuals, with very rare temporary exceptions in the stocks we have employed, orient upward upon a surface tilted at 15° to 20° to the horizontal or greater. In a ventilated dark room where the temperature is 22° to 25° observation is possible either under non-stimulating red light of low intensity, or (in darkness) by means of markings with luminous paint. Accurate record of the paths pursued is possible by the procedures earlier given (Crozier and Pincus, 1926-27, *b, c, d, e*). At any one inclination the probable error of the mean θ is quite small, if due attention is given to the selection of the litters at the start, and to their intermittent replacement with the mothers for feeding. Just preceding the opening of the eyes the variability of behavior is definitely increased; this is seen also in mice. Low intensities of white light, before or after the opening of the eyes, render it very difficult to secure uniform orientations. This effect cannot be obviated by surgical removal of the eyes, which enhances irregularity of creeping and accentuates pauses; in these animals, as appears to be the general rule, geotropic orientation is evident only *during* progression. The consequence of removal of the eyes is undoubtedly comparable to that seen in certain fishes (Crozier, 1918). The desired test can however be made in another way, with young mice (Keeler, 1927-28). With these, there is available a gene which prevents the development of the retinal rods; with certain possible rare exceptions, mice carrying this gene are blind, and can be used for the geotropism experiments in daylight (Keeler, *loc. cit.*).

As a sufficient approximation, θ is found proportional to log sin α. The graph is more exactly a long-drawn "*S*," however, as every series of experiments has shown. The logarithmic fitting is sufficient, none the less, for certain purposes. More exactly, considering the way in which the weight of the body is carried by the legs, it is possible to show that cos θ would be expected to decline in direct proportion to sin α. This expectation is precisely obeyed, and the constants in the resulting equations are recoverable in new tests with rats

of the same stock after an interval of over a year (Crozier and Pincus, 1927-28). The data from such experiments can be submitted to a variety of independent tests, which can only be very briefly referred to here. The *speed* of upward progression obeys the same rules as θ (Pincus, 1926-27); from this fact, computations can be made as to the rate of doing work when the body is lifted, as a function of α. The effect upon θ of adding weights, attached to the root of the tail ,which is predicted in the derivation of the relation $\Delta \cos \theta / \Delta \sin \alpha = -$ *const.*, is also found experimentally (Crozier and Pincus, 1926-27, *b*; Pincus, 1926-27). The *variability* of θ at successive magnitudes of α shows that the measurements contain within themselves an automatic check upon their significance. If θ is determined by α in the manner indicated, increasing sin α should *decrease* the variability of θ according to the same rule as that by which θ itself increases. This is due to the fact that increasing geotropic excitation not only involves a greater amplitude of response, but proportionately cuts down the effects due to unavoidable sources of stimulation not directly concerned with the experiment. We find, in fact, that the statistical measure of the variability of θ decreases in direct proportion to log sin α. Moreover, if this conception be sound, the *rate* of this decrease should depend directly upon the number of individuals involved (the total number of observations of θ being the same in each case). A litter of two individuals indeed shows one-half the rate of decreasing *C. V.* θ as log sin α increases, as is shown by a comparable one of four individuals. The way in which this kind of treatment can be developed for the analysis of a particular sort of variability of behavior deserves emphasis, but cannot be extended here.

Three further checks upon the significance of the measurements remain to be considered. One of these has to do with orientation in compound fields, and is reserved for the moment. Of the other two, one is of a qualitative character, and hence especially valuable, while the last will serve to show the manner in which the results may be employed for purposes of additional investigation.

The mathematical formulations of geotropic orientation thus far presented are either such as may have a real meaning, or else they must be the happy results of a species of accident in some way due to the averaging of observations. If the latter were true, then it is scarcely conceivable that animals creeping in quite different ways, in which the possible manner of support of the body upon an inclined surface is completely different, should provide specifically different empirical relationships between θ and α, or obey the diverse rational equations. Several other cases may be mentioned. The beetle *Tetraopes tetropthalmus* has been extensively studied by Crozier and Stier (1927-28, *b*). The body is in contact with the plane of creeping

at the tips of the legs and at the posterior end of the abdomen; θ is found proportional to sin α directly, *not* to log sin α. The complete formulation for this case is difficult, and I need not here consider it further. Essentially, the magnitude of θ is determined by the fact that the leg muscles not only support the downward pull of the mass of the body, but are also subject to a torque due to the twisting movement of the abdomen, the center of gravity being posterior to the point of attachment of the legs. This interpretation is readily controlled by the attachment of additional loads (wax) to different parts of the beetle's body. When a mass of wax is attached to the abdomen, θ is increased. If attached to the anterior end of the body, θ is *decreased*, although the total load carried has been augmented, because the torque due to the abdomen is reduced. Again, with these insects, it is possible to perform the converse experiment and to *reduce* the load carried, and at the same time the torque on the legs, by amputating the abdomen. If our understanding of the situation is sound, θ should then be decreased, which is in fact the case.

These instances, and a number of others, can thus be understood as manifestations of geotropism which depend upon the distribution of the pull of the organism's mass upon the supporting musculature. The analysis does not exclude the involvement of statocyst function in geotropism, but leaves it to be demonstrated just what this function may be in specific instances. (*cf.* Arey and Crozier, 1919, 1921; Cole, 1925-26; Crozier and Federighi, 1924-25, *c*). Related to this matter is the failure to obtain clean-cut relationships between θ and α in the case of slowly moving marine and fresh water gastropods, as we already know; but this point cannot be developed at present. I must also refrain from going into the very attractive questions arising when geotropic orientation is reversed.

Other animals have been examined for the control of geotropic orientation, among them blow-fly larvae and fiddler crabs, in experiments by Dr. B. Kropp, and *Drosophila* (in part by Dr. K. Hirasaka), and certain species of ants (by Mr. T. C. Barnes). I need refer only to two of these. The case of *Drosophila* shows θ very nearly proportional to sin α. Dynamically, from the conditions determining stability of the animal when creeping with the body freely supported on the legs and out of contact with the plane, we should expect sin θ to be a rectilinear function of cot α, and this relationship is found to be satisfied. These tests are interesting for points in methodology which they involve. Flying is prevented by introducing in the desired stock the gene *vestigeal*, which prevents development of the wings. The effect of light may be practically excluded by use of the gene "*eyeless*." And the formula may be tested by employing a mutant stock in which the lengths of the legs

have suffered genetic reduction, *dachs*. The latter "genetic operation" accomplishes a result impossible to attain by direct surgery.

The orientation of fiddler crabs I may take as a final illustration of the fact that the empirical and the rational formulae applicable for mechanically diverse instances of geotropic orientation cannot be regarded as "accidents," and that, in consequence, the muscle tension theory of the limitation of orientation is not merely adequate but necessary. *Gelasimus* creeps not straight forward, but sideways. In the dark, Dr. Kropp found θ very closely proportional to sin α directly. If this is due to the attainment of mechanical stability, and thus to the release of a certain class of tensions on the legs, θ should be altered if the center of gravity is shifted. It happens that nature has performed the experiment. The large claw of the male fiddler may be carried "up" or "down" as the animal creeps sideways on an inclined plane. In the former case, θ should be less, at each value of α, and this is the fact. Removal of the claw abolishes the asymmetry, which does not appear in the symmetrical females. It will be noticed that the proportionality of θ to sin α, in one class of cases, and to log sin α in the other, is a parallel, but no more, for the two classes of cases in phototropism.

I have already referred separately to two facts which it is necessary to bring into conjunction in dealing with a final aspect of geotropic orientation. One is the fact that in the case of rats and mice the graph of θ *vs.* log sin α is *almost* a straight line, but, in reality, a long-drawn *S*. The other is the necessity for genetic uniformity in the test material. This is proved by the fact that whereas the same values of the constants in the equations are repeatedly obtained with individuals from the same stock, it is neverthless an easy matter to obtain separate stocks, of known different genetic history, and distinct species (Crozier and Pincus, 1926-27, *e*), in which (1) the slope of the line connecting cos θ with sin α is different, or (2) in which the slope is sensibly identical but the intercepts on the axes are different. I cannot here enter upon the possible deductions from these facts, which Dr. G. Pincus has obtained in very extensive trials, but their genetic utilization (*cf.* Crozier and Pincus, 1926-27, *e*) is obvious; Dr. Pincus has already obtained very suggestive results in this connection. The question arises: How is it possible to regard the inheritance of the constants in an equation? This brings us back to our long-drawn *S*. Briefly, we may view the attainment of geotropic orientation as governed by the animal's turning until the difference in the forced tensions on the supporting muscles of the two sides of the body is reduced to such a level that not more than a certain number of tension-receptors are stimulated in excess on one side more than on the other. Certain calculations can be made on this basis. The number of such (groups of) tension recep-

tors may very well be subject to inheritance. The matter is of course complicated in several ways which I need not now consider, but it appears to open a road to the investigation of the physiology of tension receptors in the intact organism, as well as of concrete aspects of inheritance in relation to behavior.

VI

Granted that under certain conditions it is possible to account quantitatively for elementary manifestations of geotropic and of phototropic orientation, I desire now to treat in a summary way several specific applications of the analyses. These consist, in a sense, in testing one's ability to predict quantitative features of response in situations such that both phototropic and geotropic orientations are in evidence. Conceivably this sort of inquiry might ultimately be pushed to include the nomographic representation of determined conduct when such variables as gravity, light intensity, wave length, galvanic current, temperature, oxygen tension, time, and other conditions can be controlled. Thus far, the data do not exist for such an extension and the simpler instances about to be mentioned are, from an experimental standpoint, difficult enough.

With rats it was possible to show (Crozier and Pincus, 1926-27, *d*) that, from previous knowledge of the geotropic orientation and of the phototropic orientation (in the case of animals with unopened eyes, Crozier and Pincus, 1926-27, *c*), it was to be expected that the intensity of light required to effect a definite phototropic counter-balancing of the geotropic orienting influence should be a power function of the sine of the angle of inclination of the creeping-plane. This prediction was found to be verified. In addition, it was possible on the same basis to account for the changing variability of the measured values of I as α was increased. To this case may be added the result of compounding galvanotropic and photic excitations, previously referred to. I desire to give more attention here to a different type of experiment, in which time enters as an additional variable. It illustrates not only the general problem of orientation in compound fields, but also the way in which the results of this procedure lend themselves to a more penetrating treatment of mechanisms of excitation.

Again, it is necessary to stress the fact that organisms and individuals, exhibiting the response desired, must be selected if properties of the response are to be investigated. The experiments involve chiefly the effect of phototropic excitation upon the direction of gravitationally oriented creeping of the slug *Agriolimax*. The operative problem first consists in securing a measure of the orienting effect of light, with the gravitational influence held constant. In darkness a negatively geotropic *Agriolimax* upon a vertical surface creeps

straight upward. When light (horizontal rays) falls upon one side of the animal, previously dark adapted, it continues to creep upward but at the same time away from the light, the path departing from the vertical by an angle β. Time enters as a variable, since photic adaptation is fast. While it would be preferable in some respects to measure at each chosen moment the intensity of light necessary to produce a constant deviation of the path from the vertical, it has been easier to measure the deviation itself as a function of time (Wolf and Crozier, 1927-28, *a*). Two forces are apparent in such experiments. The light leads to photonegative orientation, and the geotropic stimulus forces the slug to creep upward. If, during exposure, the test-light be removed, orientation is at once upward. If on the other hand the inclination of the creeping plane be decreased, orientation is more completely away from the light. Therefore the path actually followed depends upon the effects of concurrent continuous stimulations of the two kinds. The geotropic stimulus may be regarded as constant. Then the angle β depends on the relation $\tan \beta = \frac{L}{g}$, where L is the phototropic vector. We may assume that at any moment the photic irritability is proportional to an amount of photosensitive material S, only very slowly produced by the processes underlying dark adaptation, and that the photolysis of S adheres to the kinetics of a first order reaction (*cf.* Hecht, 1922-23; 1923-24; 1924-25). This means that over any short interval the decomposition products formed from S, which are responsible for excitation, will be proportional to S_t and thus to $S_0 e^{-Kt}$, when S_0 is the amount of S at complete dark adaptation. Inserting this relation in the previous equation, $\tan \beta$ should be proportional to e^{-Kt}, and $\log \tan \beta$ should be directly proportional to $-t$. Moreover, the rate coefficient of light adaption, K in the preceding expression, would be expected to be practically proportional to the logarithm of the intensity of the light used to produce adaption. Both of these expectations are abundantly verified. They are further checked by the orientation-effect with forms only *slowly* adapting to light (*cf.* Yagi, 1927-28, as in *Limax,* where experiments by Cole and Crozier have also studied β when the vectors g and L are not at right angles. Since β decreases with time, it might be suspected that the slug "learns" to overcome the effect of the light. This would suppose that the concomitance of geotropic and phototropic excitations produces, centrally, a "block" for photic impulses (or else, enhances the geotropic effect). This would be interesting in itself, if verifiable, since the process clearly obeys a definite law. The alternative interpretation is that the photic adaptation is direct, and occurs in the eye. By means of experiments in which the animal creeps on the vertical

plane only during the brief interval required to measure β, the exposure to light being continuous, it is shown (Crozier and Wolf, 1927-28) that the measurable adaptation does not depend upon the simultaneous competitive action of the geotropic stimulation; it must therefore be regarded as located in the eye.

The notion that excitation by light is a photochemical matter, and that the maintenance of photic reactivity and its recovery after exhaustion may be in some fashion connected with a reversible photochemical receptor system, seems to have been held in a very general form since Hering. Growing knowledge of the chemical effects of light made it attractive to translate such ideas into photochemical terms. An early attempt to treat the question precisely and specifically, but with some important deficiencies, was made by Müller (1896). To explain the simultaneous exhibition of capacity to react to a sharp decrease in light intensity and also to be excited to phototropic movements by the continuous action of light, as seen in the integument of certain holothurians, it was suggested that the same photochemical system, if part of a reversible reaction of a certain kind, might be conceived to serve for both modes of stimulation (Crozier, 1915). Hecht has been able to achieve the step-by-step proof, in a very satisfying and convincing manner, that a particular type of reversible photochemical system underlies photic excitation of *Mya*, *Ciona*, *Pholas*, and the human eye. The properties of this system permit the prediction and the rational quantitative interpretation of new and unexpected characteristics of the stimulation process. A major property of the underlying mechanism is adequately expressed (Hecht, 1918-19; 1922-23) in the following paradigm, where S represents primary photosensitive substance, P and A, two of the products of its photolysis: $S \underset{\text{dark}}{\overset{\text{light}}{\rightleftharpoons}} P+A.$

The system is reversible, and with time, under continuous constant illumination, there is established a stationary state ("equilibrium"). The "dark" process, of which the kinetics serves to describe the course of dark adaptation, is definitely bimolecular (Hecht, 1926-27), with high temperature characteristic; the "light" reaction is of first order, and negligibly influenced by temperature (Hecht, 1919-20, *a*).

These relationships have been established most elaborately by means of experiments with organisms in which the speed of response following the delivery of a measurable quantity of light gives data necessary for the quantitative treatment, and in which light adaptation is rapid and photic excitation therefore necessarily discontinuous. To carry over such considerations to the analysis of continuous excitation, as in phototropic stimulation, it has been necessary to find

phototropic organisms in which the rate of light adaptation is measurable, neither inconveniently rapid nor too slow. The photic adaptation of *Agriolimax* has been followed by causing the phototropic influence of the light to work against the "brake" provided by a vectorially constant excitation of a different kind, namely that due to gravity, acting at right angles to the phototropic vector. The function of such a "brake" in contributing to the significance of the measured orientations has been discussed previously in relation to circus movements (Crozier and Federighi, 1924-25, *a*). Data necessary for treatment of the light adaptation are gotten from measurements of the resultant angles of orientation as related to time and to intensity (Wolf and Crozier, 1927-28). Such measurements are possible because the rate of light adaptation, especially at temperatures in the neighborhood of 15° or slightly above, is so very much faster than that of dark adaptation, as adequate tests showed. It was easily established that the rate of light adaptation is very little influenced, if at all perceptibly, by temperature; and that its course follows that of a first order reaction in which the the "velocity constant" is a linear function of the logarithms of the light intensity, as already indicated. These phenomena therefore parallel, in an exact manner, the properties of the "light" reaction already discussed.

The interpretation of dark adaptation requires measurements of the photic excitability at sucessive intervals following exclusion of light from individuals previously light adapted. To reduce the probability of adventitious errors of estimation the experiments were made at a temperature high enough to bring the time for practically complete dark adaptation down to a little over one hour. At about 12°, the time required may be as long as 3 to 4 hours; at 20.5°—22°, the temperature prevailing throughout the present experiments, the time is less than half of this. Since light adaptation, *as measured by the power to influence geotropic creeping on a vertical surface,* is effectively complete within about 5 minutes, as a rule, with high intensities, the experiments must, for simplicity, take the form of establishing curves of light adaptation as affected by known preceding periods in darkness. From such data it is required to deduce the ideal photic excitability at the last moment in darkness, before the exposure has begun, or in other words the kinetics of the "dark" reaction (Crozier and Wolf, 1927-28, *b*).

The simplest assumptions which serve to explain the data are the following. We suppose that the excitation at any moment is proportional to $-\frac{ds}{dt}$. The intercepts on the $\log_{10} \tan\beta$ axis at zero time of exposure to the light give measures of $-\frac{ds}{dt}$ at

the first instant of exposure. The ratio $\tan\beta_0 / \tan\beta_t$, labelled R, where β_0 is the intercept angle for fully dark adapted slugs, permits one to manipulate the resulting equations with somewhat greater ease. At different levels of dark adaptation the formation of sensitive material must be taken into account. Various simpler hypotheses do not agree with the facts so far obtained, and the "dark reaction" in question has to be taken as a second order reaction with positive autocatalysis. The differential formula for such a process is:

$$\frac{ds}{dt} = [\ K'+K_2\ (S_0-x)\ ]x^2,$$

where S_0 = concentration of S at complete dark adaptation, x = concentration of P or A, and K' and K_2 are velocity constants. It is usually sufficient to neglect the constant K', which in most autocatalytic systems appears to be very small (*cf.*, however, Crozier, 1926-27, *a*). The photolysis of S requires $-\frac{ds}{dt} = K_1\ (S_0-x)$. When both processes go on together,

$$-\frac{ds}{dt} = K_1S_0 - K_1x - K_2S_0x^2+K_2x^3.$$

The integral equation to which this gives rise is superficially formidable, but a variety of simple consequences at once derive from it and are found to correspond with directly ascertained experimental data:

$$-t= \frac{1}{K_1-K_2S_0^2}\left\{ \frac{1}{2}\log \frac{K_1S_t^2}{S_0^2\ (K_1-K_2S_0^2 + 2K_2S_0S_t - 2KS_t^2)} + \frac{K_2S_0}{\sqrt{K_1K_2}}\tan \frac{h^{-1}\ K_2(S_0-S_t)}{\sqrt{K_1K_2}} \right\} .$$

For example, the second derivative of this expression gives the way in which the ratio R should change with time of dark adaptation; it should be found that $1/R$, plotted against time, is a sigmoid curve with an inflection point occurring when the dark reaction is just a little less than 0.67 completed. The experimental value is 0.64. The rate of light adaptation at different levels of dark adaptation is given by $-\frac{d^2s}{dt\ dx} = K_1 - 2K_2S_0\ x + 3K\ x^2$, and this must pass through a *minimum* value at a period of dark adaptation corresponding to the point at which the curve of $1/R$ is inflected. This is true experimentally. Moreover, for early stages of light adaptation, at any level of dark adaptation, as in the present measurements,

log tan β (*i. e.*, log S,) should be linearly related to $-t$, since K_2 is known to be much less than K_1. Other deductions might be mentioned, but enough has been said to indicate the way in which the results of compounding tropistic effects can be employed, in this case, as a test of the tropism concept and for the development of its utility.

VII

A major aspect of the theory of animal conduct is concerned with the initiation and the control of directed movements. The tropism doctrine successfully describes the course of one general class of such directed movements. For phototropic movements and for geotropic orientations the description may be precise, physically interpretable, and is sucessfully employed for the prediction of movements in compound fields of excitation involving both photic and gravitational stimulations, even when time also enters as a significant variable. The theory of photic orientation, traditionally based upon rectilinear loci of progression, is moreover easily extended to deal with curvilinear paths, such as result from the concurrent operation of postural reflexes during orientation or from the effects of non-parallel rays of light. The additional fact that the basic resolution of certain of these questions was started on the foundation of experimental results obtained with mammals, later to be successfully employed for the analysis of corresponding modes of behavior among invertebrates, puts a somewhat different emphasis upon the tropism doctrine from that possible several years ago.

The tropism concept has nothing to say regarding the type of contrast in behavior which appears when an organism may be moving about at one moment, totally quiescent at another, with external circumstances to all seeming identical. Nor does it deal directly with such matters as experimental or "spontaneous" reversal of sign in orientation, although these phenomena it may utilize. Its primary concern is with the idea that when the excitations are equivalent upon the two sides of an organism structurally or dynamically symmetrical, orientation ceases. From this assumption equations follow which describe the loci of movement under particular conditions, even when two contrasted sorts of stimulation produce a conflict which the organism must resolve. Constants in the equations are provisionally assumed to be measures of properties of the adjustor mechanisms, which may then be studied from the standpoint of their experimental modification, with the hope of approaching in this manner some comprehension of the character of the mechanisms thus recognized. Such investigation appears to give a means for the study of central nervous functions not otherwise measurable.

The history of the discussion of the quantitative investigation of

tropisms has been confused by disagreements as to the exact steps in the process whereby an organism becomes oriented, and, in another respect (more recently), by a rather unnecessary complication of descriptive terminology. An interpretation sometimes, if not usually, conceived to be in profound opposition to the tropism concept, in its primitive form runs as follows: at any moment the sum total of molecular changes in an individual creates a specific state, a *physiological state*; alteration in the energies of the environment constitutes, or may supply, a "stimulus"; passing over certain difficulties which confront this definition of a stimulus, we grant that, as a result of the stimulus, movements are executed; while these movements, or indeed their exhibition in any case, are in certain respects conditioned by the aforementioned specific state and by those structural conditions which mechanically permit only certain sorts of movement to appear, they are in essence to be conceived as movements without external constraint as to *direction*—that is, they are *random movements*; by successive trial among these, the organism arrives at a position in which excitation is no longer experienced. There is to be included here also the origination of movements from stimulation arising within the organism, which consequently appear as *spontaneous movements*, and which seem usually regarded as devoid of constraint as regards their incidence in time.

We may with justice refer to the curious fact that, in the heated discussion formerly prominent in relation to this hypothesis, few if any investigators have considered it worth while to examine the quantitative aspects of such random movements. That it should be especially important to do so is clear, since (*cf.* Parker, 1920) the appeal to "physiological states" not precisely characterized can so easily lead to an unfortunate mysticism, and because the theory of random movements implies a hopelessly haphazard connection between stimulation and activation of motor organs, such as should vitiate any possibility of regularity and predictableness in animal conduct, and is to this extent discordant with experience under conditions where prediction might be reasonably looked for.

Everyone knows that "aimless" and spontaneous motor activities are a feature of the conduct of animals. The real questions reduce to these: what rôle, if indeed any, do random movements play in tropistic orientations? and, are random movements following excitation, spontaneous movements, and fluctuating "physiological states" such as govern variability of response, really lawless? The first question we consider to be already sufficiently answered. To the second there has been as yet no general answer. The control of rhythms of movement by temperature is particularly suggestive in this connection. Similar changes in "sensitivity" complicate certain modes of tropistic response, but give every evidence of being anything but lawless (*cf.*

Crozier and Stier, 1927-28, *b*). Long-period rhythms of activity in marine organisms, and diurnal variations in reactions, have indeed been described which seem independent of persisting causal rhythms of excitation. And some years ago K. Przibram endeavored to relate the chance movements of *Paramoecium* to the formula for Brownian motion. A different line of approach has been followed by Richter (1927). But there has been no treatment of such aspects of behavior as would be required for development of a theory as to their nature and the mode of their influence in producing variability in behavior. One category of "variabilities" has been discussed at several points in the present paper. A further type is illustrated by the contrast between quiescence and movement, of which an extreme although general instance, at the same time experimentally manageable, is given by the phenomenon of *tonic immobility* ("death feigning"; *cf.* Crozier, 1923; Crozier and Federighi, 1923; Hoagland, 1927) evident in a great diversity of animals.

I mention these considerations in order to show that the tropism concept need not be supposed or expected to explain everything. It remains definitely justified and of increased power, within the sphere it was originally designed to occupy.

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THE HYDRAULIC PRINCIPLES GOVERNING THE FUNCTION OF THE COCHLEA*

From the Psychological Laboratory of the University of Missouri

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ABSTRACT

General equation. Rejecting all *fantastic* "anatomical" data, that is, not making any anatomical assumptions but those of facts which are clearly visible under the microscope, but, on the other hand, taking *conscientiously* into account everything that can thus be seen, we derive a general equation describing the hydraulic functions of the mammalian cochlea.

Statement of the chief aim of the theory. Whenever anything occurs on the outside which deserves to be called a sound in the sense of the physicist, and which varies more or less periodically with the *time*, we want to know first, what *length or lengths* of the phragma will be involved, because this determines *the number of sensitive cells stimulated* and thereby the magnitude of the nervous flux (or "the sensation intensity," loudness); and we want to know second, how *frequently* per time unit these various lengths of the phragma are shaken, because this determines *the nature of the stimulation and accordingly the quality of the chemical process* resulting in each of those sensitive cells (or "the sensation quality," pitch). Thus, our most important aim must be the establishment of a relation between *l* and *t*.

Solution of the equation. The equation is solved *in special cases* for twofold verificaton: (1) by auditory observation; (2) by experimentation on a large, transparent, hydraulic model.

Thirty years without adverse criticism. The hydraulic theory during its life of thirty years has received some praise by the few who were willing to read it, has been viciously misrepresented by some who would not or could not comprehend it, has been innumerable times dogmatically rejected by those who did not even know what it consists in. We invite serious criticism of its basic assumptions, its mathematical analysis and, last but not least, its agreement with the facts of hearing.

The concept of a "traveling" bulge is not a part of the hydraulic theory. The author requests textbook and magazine writers to cease misquoting him. He has never either enounced or supported any "traveling" bulge theory.

A tube of solid walls, whose total length does not particularly interest us, and whose cross-section (peripheral wall) does not particularly interest us either and may therefore in our mathematical analysis be assumed to be uniform throughout, is by a flexible phragma lengthwise divided into two parallel halves.

It is this *phragma* that we are mainly interested in, because well protected in its interior it carries *the sensitive cells whose chemical excitations* are somehow propagated along *the nerve fibres*.

In order *to resist hydraulic pressure, the phragma, as is to be expected, is well anchored* at the side in the following manner. The

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cross-fibres of a relatively thin and especially tough[1] part of the (in its totality very thick) phragma are *frayed out before they* reach the solid wall of the tube. Entirely unlike a stretched string in a musical instrument, which because of having permanent tension must go straight and solid to its single point of attachment, these assembled fibrils of a living fiber *turn* up and down variously and then only attach themselves *curvedly and separately* to the bony wall, analogous to two curved *anchor chains* used for holding the bow and the stern of a ship in place against the alternately running tides. The reader of course must with us compare, say, the mast of

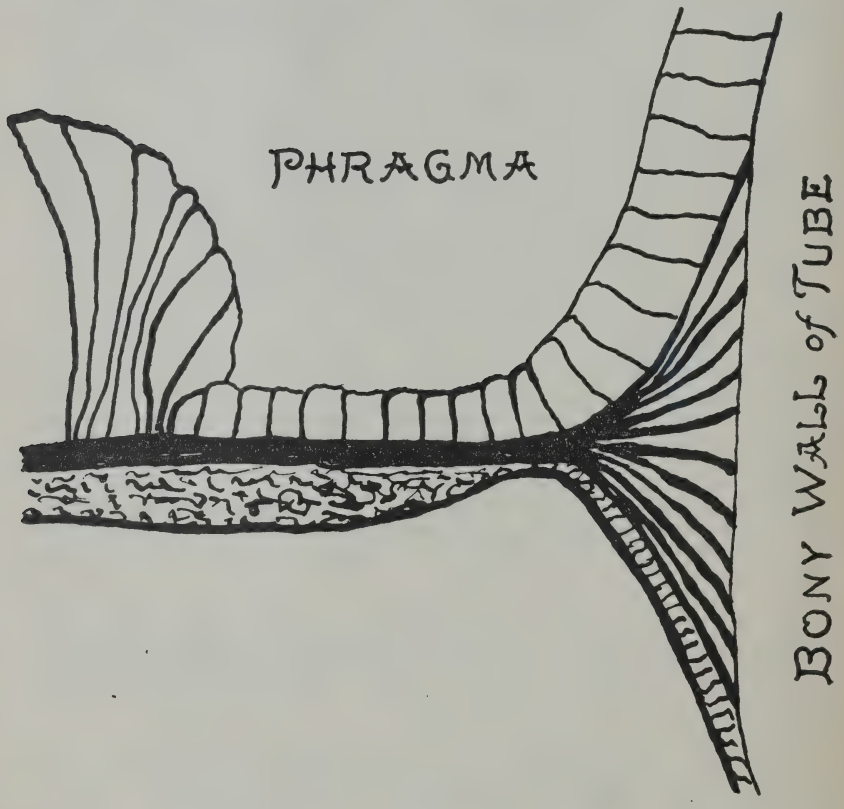


FIGURE 1

THE METHOD OF ANCHORING (NOT STRETCHING) THE BASILAR MEMBRANE TO THE BONE

[1]It is, later than all other parts, broken down by abuse. Compare Yoshii, *Zsch. f. Ohrenheilkunde,* 58, 1909, pp. 201-251.

that ship with the unfrayed part of the phragma's fibrous structure, the bottom of the sea with the bony wall of the cochlea tube, and *the two* anchor chains with *the very numerous* fibrils into which a single cross-fibre of the phragma is frayed out. Such is *the real anatomy,* too readily forgotten by us after we have been filled from early youth with the dogmatic belief that the creator has put "a harp" in the inner ear. That such a belief can wax incredibly strong appears when it accuses those who endeavor to *stick to what the microscope shows and mathematics deduces* of being speculators.[2]

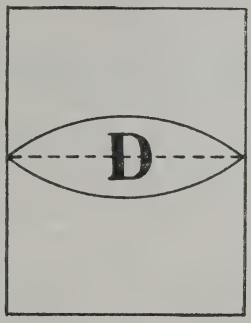


FIGURE 2

POSITIONS OF THE PHRAGMA

In real anatomy this phragma (its thickness measured between the limits of Reissner's membrane and the vascular layer lining the basilar membrane, its width from bone to bone) is almost as thick as it is wide and is of a very spongy nature, more so than Swiss cheese, consisting of far more fluid than of solid substance. But since we are interested only in *the behavior of a single line drawn within the phragma from one side* of the tube to its other side, and in the behavior *of a second line, perpendicular to the first and drawn*

[2]For example, Wilkinson, G. W. Is the Question of Analysis of Sound by Resonance in the Cochlea Still an Open One? *Amer. J. Psychol.*, 38, 1927, page 265, lines 23 and 34: "The alternative theories — — — are purely speculative. Whatever evidence we possess is against these possibilities." After using such strong words as "purely" and "whatever" the author quoted *does not specify or quote* a single "evidence" against that one among "the alternative theories" which concerns us in the present article, the hydraulic theory rejecting permanent tension, although it has attained the ripe age of thirty years; its *very diversified beginnings* going back to Pierre Bonnier in 1895 on its *purely morphological* side and to Max F. Meyer in 1896 on its *purely analytical* side, first combined into a rational unit by Meyer in 1898 and developed by him in more than a dozen further publications, none of which has ever received any adverse "criticism" except "the cold shoulder" of ignorance.

lengthwise through the phragma, we might for the development of the mathematical theory as well think of the phragma as if it were simply like a ribbon of chamois skin.

If the pressure of the fluid is different in one of the halves of the tube from what it is in the other, the phragma must by physical necessity assume a concave form toward the greater and a convex form toward the lesser pressure. In Figure 2 we show diagrammatically first the cross-section of the tube *as if* it were a rectangle, second the *average* position (thesis) of the phragma as a dotted line dividing the tube into its halves, and third *the area* described by the phragma in exchanging its convex and its concave positions as an area which, because in a sense it is a *double* area, we have named D. In order to have a briefer term by which to refer to "the average position of the phragma," let us introduce for it the term "thesis." (In addition to speaking of the "thesis" of the phragma, we shall later have to speak also of its "parathesis" and its "hyperthesis.")

Anatomy reveals that the width of the phragma, in mathematical abstractness represented by the dotted line of Figure 2, is not uniform throughout the tube, because in certain regions of the tube it is partially replaced and thus narrowed by *inflexible* material (bone) protruding into the tube from the hard walls. D is therefore a mathematical function of l, if we mean by l the measurement of that part of *the length* of the tube with which we are concerned, measuring it from the beginning of the tube and phragma to whatever point of the tube and phragma is in question. D is also, of course, a function of the pressure difference already hinted at. We therefore consider D in the form of the following equation:

$$D = f\ (\ l,\ p) \qquad (1)$$

The phragma being of a leather-like substance, it is clear that it offers to the pressure which translates it beyond its thesis (the dotted line) almost no resistance as long as this translation is below a certain "distance" which we shall name the phragma's *parathesis,* and that it offers to the pressure almost infinitely great resistance as soon as the translation has considerably progressed beyond the parathesis, that is, to a "distance" from the thesis which we shall name the *hyperthesis* of the phragma. (For a concrete example, think of what happens when a person sits down on a cane-seated or leather-seated chair.)

We need a mathematical function which describes a behavior of this kind with serviceable approximation. Such a function is, e. g.,

$$\text{Resistance} = y = \frac{C}{\operatorname{cotg} x - \cos x} \qquad (2)$$

For small values of the translation, which in equation (2) corresponds to x, y must by trigonometrical necessity be virtually zero

since the cotangent is then near infinity and the cosine is anyway strictly limited.

For values of x which have passed the octant of the circle the resistance y must rapidly approach infinity, for the denominator differs then more and more slightly from zero.

Thus only within a certain limited range of the possible translation of the phragma is it of any use to speak of anything like "proportionality" of pressure and resistance to pressure. This range of the translation of the phragma we conceive of as the range from the parathesis to the hyperthesis. Figure 3 illustrates this.

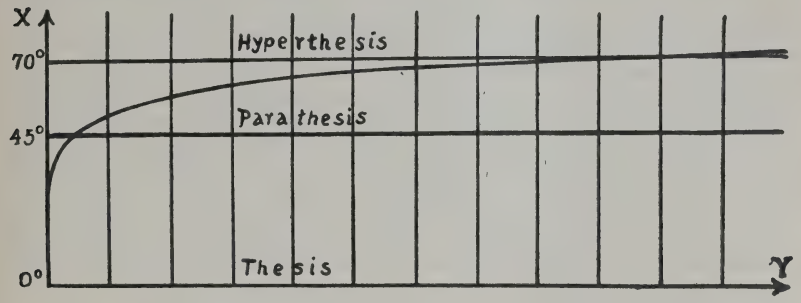


FIGURE 3

RESISTANCE TO PRESSURE

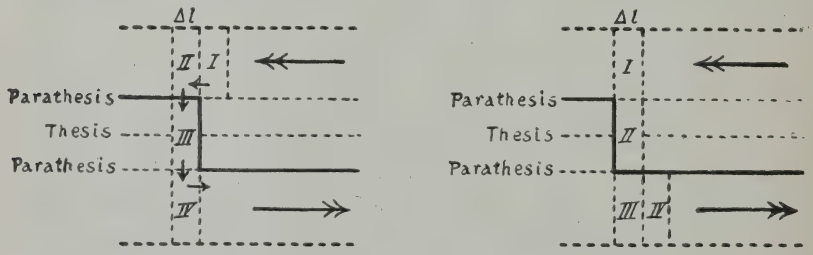
The pressure difference in the two halves of the tube results from the effect of two piston-like[3] structures with which each half of the tube is equipped at the beginning of the tube, which in our following figures we shall represent as located on the right side of the paper.

Let us now imagine the occurrence of a pressure difference, positive in the upper half, negative in the lower half. The result then is a displacement of *the fluid columns near the beginning of the tube*,

[3]A great deal of publication exists by the engineer Sir Thomas Wrightson about his "discovery," for which in my opinion he has been needlessly praised by the anatomist Sir Arthur Keith, of a special significance of the fact, that the stirrup does not make exactly the movements of an ordinary cylinder piston, but rather those of a "hinged" piston which suffers in every translation also a slight rotation. I cannot see that this fact, which I readily admit, makes any theoretical or practical difference to the hydraulic function of the cochlea; but someone else may see it, and I therefore quote it because neglect of the truth has too long been the bane of physiological acoustics both in monographs and textbooks. Let me mention by the way that I perfectly agree with Sir Arthur Keith in considering the existence of the hammer and the anvil as *not essential* to hearing and to any significant feature of its theory. Compare my textbook, The Psychology of the Other-One, p. 298 ff., where the evolutional significance of the auditory ossicles is clearly described.

toward the left (and within limits also down) in the upper half of the tube and toward the right (and within limits also up) in the lower half of the tube. In Figure 4 is shown a vertical cut passing through the axis of the tube. Δl is an increment of the *length* of the tube. For simplicity's sake it is assumed that at the moment t the phragma is in its upper párathesis on the left side of Δl and in its lower *parathesis* on the right side of Δl.

(The writer after many years of experience in teaching this subject matter, is accustomed to having a student ask: "But why don't you assume on the left side of Δl the phragma to be *in its thesis*?" The answer to this naïve question is simple: "In a *periodic and long continued* function (over many, many periods), an assumption like that would not serve any useful purpose, would only complicate and obscure the discussion." An analogous case would be that of a botanist discussing the alternate effects on a plant of daylight and darkness of the night. He would of course start the discussion with *either* the effect of light *or* the effect of darkness. He would *not* start the discussion with the effect of "twilight," as if, although light is said to follow, it could still be debated whether this twilight was preceded by the effect of day or of night. In such a debate only "philosophers" delight.)



FIGURES 4 AND 5

FIG. 4. BEFORE Δt. FIG. 5. AFTER Δt.

During the time Δt the following events occur. The fluid quantity represented in Figure 4 by the rectangle I is forced to take the place of II. The fluid quantity II moves down and takes the underlying piece of phragma along with it. The fluid III replaces IV. And the fluid IV becomes a part of the fluid column which is in motion toward the right. Figure 5 shows the result registered at the moment $(t+\Delta t)$.

It is obvious that the fluid quantity I (or II or III or IV), displaced during Δt is proportional to the variable D as previously defined, that it does not simply depend on an imaginary constant distance

from parathesis to parathesis, which is only a "kind of average" distance, useful for drawing diagrams like the Figures 4 and 5.

We now have to consider the work done by the pressure p during the time Δt. This work is the sum of *three kinds of work*, of *keeping* the fluid columns in motion against their internal friction, of *placing* the fluid quantity II of Figure 4 into its new position against the resistance of the phragma, of *having to leave*, with the newly acquired velocity $\pm \frac{\Delta l}{\Delta t}$ the two quantities which were added (one to each) to the two already moving columns of fluid.

(It should be mentioned here that in the next paragraph the *friction at the wall* will be regarded as invariable because the tube tapers little and, furthermore, is very spacious compared with the minute volume of fluid which at a maximum moves through the tube per unit of time. Also *the density* of the fluid will be regarded as constant, since the dimensions of the total volume of fluid are small in comparison with the wave-length and the maximum pressure change in time. Taking these factors into account as variables, seems to make too little difference to our problem.)

The counterpressure p_1, due to *internal friction* in the fluid must be proportional to three factors, the viscosity coefficient naturally, further the double sum of the products $D\Delta l$ from $l = 0$ to $l = l$, because this sum determines how much mass is affected by the viscosity coefficient, and to Δl because this increment determines the "velocity" of the mass during Δt, i. e., the average number of fluid molecules having to pass each other.

It is unfortunate that the anatomists give us scant information with respect to the normal viscosity of the fluid. And even with respect to the function $\frac{\delta D}{\delta l}$ they give us only inexact information.[4]

Equation (3) then gives us that part of the work done by the external pressure p which serves to keep the two fluid columns in motion against retardation by their internal friction during the time Δt.

[4]The most recent measurements, apparently more exact than all previous ones, of the width of the phragma from the beginning to the end, showing that it first increases and then again decreases, are those of S. R. Guild, *Science*, Jan. 21, 1927, "The Width of the Basilar Membrane." Certain textbook writers who believe in "a harp in the ear" will be astonished at this curious arrangement of the strings of this "harp," which has its longest "strings" *between* shorter ones. These measurements are not made on human cochleas, but on cochleas of guinea pigs; but they permit probable conclusions with respect to the human cochlea.

$$p_1 \Delta t = \left[\frac{\Delta l}{\Delta t} \, 2\eta \sum_{0}^{l} D\Delta l \right] \Delta t \qquad (3)$$

Secondly, we wanted to determine the work done in placing the fluid quantity II of Figure 4 into its new position against the resistance of the phragma. Since we have to assume that at the upper parathesis *to the left* of Δl the "tension" of the phragma during some time just preceding the moment t has been infinitely small on account of lack of pressure, the resistance which the Δl long piece of the phragma exerts against the downward push of the pressure p must be derived from equation (2) as being equal to:

$$p_2 \, (\Delta l) = \frac{K\sqrt{D}\,\Delta l}{\operatorname{cotg} \frac{1}{2K}\sqrt{D} - \cos \frac{1}{2K}\sqrt{D}} \qquad (4)$$

for $\frac{1}{2K}\sqrt{D}$ is the translation of the phragma *from its thesis*, $K\sqrt{D}\,\Delta l$ is the surface of the phragma increment against which the pressure pushes, and K is an anatomical constant which is of course totally unknown because probably *no anatomist has even given thought to the urgent need* of determining it by actual measurement, that is, by measuring in a *living* cochlea the translation of the phragma under a given pressure difference.

[Instead of devoting themselves to such *actualities*, anatomists perform experiments with membranes which they have made of totally *foreign* materials such as rubber or phosphor-bronze and which—to bring the absurdity to a climax—they have *artificially stretched*. How easily anatomists deviate from the truth of their own science, is also demonstrated when one of them unthinkingly[6] endorses a physicist's computation done to please his friend, in which the viscosity of the cochlea fluid is regarded as equal to that of *water*. The viscosity of Le Page's glue would have been a better guess.]

The work done in placing the fluid quantity II of Figure 4 into its new position against the phragma's resistance is not, however, *the total work of this nature* done during the time Δt. We must remember that the long piece of phragma from 0 to the point l is at the moment preceding Δt really not in its parathesis, but all its increments have already been pushed beyond the parathesis where they appear drawn in Figure 4, toward the hyperthesis. The total work done by the pressure during the time Δt against the phragma's resistance must therefore be found by replacing the right side of equation (4) by *the sum of all those products, varying* l from 0 to l. We then have the following equation:

[6]Wilkinson and Gray, *The Mechanism of the Cochlea*, 1924; computation by S. R. Milner, p. 244, line 4.

$$p_2\ \Delta t = \Delta t \sum_{o}^{l} \frac{K\sqrt{D}\ \Delta l}{\operatorname{cotg} \frac{1}{2K}\sqrt{D} - \cos \frac{1}{2K}\sqrt{D}} \qquad (5)$$

Thirdly, we wanted to determine how much work is done when those two elementary fluid quantities after the time Δt have the velocity $v = \pm \frac{\Delta l}{\Delta t}$ which they did not have exactly before that time. Let us express this change in velocity by $\frac{\Delta v}{\Delta t}$. It is to be noted that for the lower one of those two fluid quantities this acceleration is negative, but Δl is also negative. The pressure lost on Δl is then as follows:

$$p_3\ (\Delta l) = S\ D\ \Delta l \frac{\Delta v}{\Delta t} - S\ D \Delta l\ \left(- \frac{\Delta v}{\Delta t} \right) \qquad (6)$$

since the mass of each of these fluid quantities must be proportional to the specific gravity S multiplied by the volume; and this volume is the product of the length Δl and the cross-section D. The quantity stated in equation (6), however, is only the major part of the pressure lost in this way.

All along the whole piece of the phragma from 0 to l, other, much smaller, pairs of fluid quantities are similarly accelerated, too, each of these fluid quantities being proportional (instead of to D) to the displacement of the phragma from a position slightly beyond the parathesis to a position further toward the hyperthesis. *Let us call this displacement* $\frac{1}{2}\ \Delta_p\ D$ *for the pressure existing at a particular moment:*

$$\tfrac{1}{2}\ \Delta_p D = \tfrac{1}{2} \frac{\delta D}{\delta p} dp \qquad (7)$$

Equation (6) then becomes equation (8), provided we also make in equation (6) the substitution $\Delta l = v\Delta t$.

$$p_3\ (\Delta l + p_3\ (l) = 2\ S\ Dv\Delta v + S \sum_{o}^{l} \Delta_p Dv\Delta v \qquad (8)$$

The total work done by the pressure during the time Δt in order to produce in the fluid columns only longitudinal velocity changes is then, meaning by S the specific gravity:

$$p_3\ \Delta t = 2\ S\ \Delta t \left[Dv\Delta v + \tfrac{1}{2} \sum_{o}^{l} \Delta_p Dv\Delta v \right] \qquad (9)$$

Statement of the chief aim of the theory: Our most important aim must be the establishment of a relation between l and t, for the following reason. Whenever anything occurs on the outside which deserves

to be called a sound in the sense of the physicist, and which varies more or less periodically with the *time*, we want to know first, what *length or lengths* of the phragma will be involved, because this determines *the number of sensitive cells stimulated* and thereby the magnitude of the nervous flux (or "the sensation intensity"); and we want to know second, how *frequently* per time unit these various lengths of the phragma are shaken, because this determines *the nature of the stimulation and accordingly the quality of the chemical process* resulting in each of those sensitive cells (or "the sensation quality").

In representing p as a function of t (in consequence of which the D of the first equation, instead of being a function of l and p, becomes a function of l and t) it is obviously most convenient to utilize Fourier's series. [To the writer this gives the additional advantage of personal protection against the misrepresentations, as violent as unjustified, of those who actually in print have accused him of attempting "to overthrow the scientific acoustics of a whole century." He has no such insane ambition, but is merely a seeker after the truth.] We shall therefore meet a Fourier's series in equation (11). Let us first write Fourier's series in its general form:

$$f(t) = \tfrac{1}{2}\, a_0 + (a_1 \cos t + b_1 \sin t) + (a_2 \cos 2t + \\ + b_2 \sin 2t) + \ldots\ldots$$

It is of the greatest importance, however, to keep in mind that *the just stated series represents the external air pressure* (density) and not our p; that our p is *the first derivative of the function describing the density*, for the following reason. Figures 4 and 5 refer purely to the pressure *difference* between the "pistons," that is, the oval and the round window. This pressure difference is due to the fact that the two windows, unsymmetrically arranged, are always at different distances from the sound plane in the external ear; but the difference of these two distances is a (small) constant. Figures 4 and 5 have reference *only* to any part of the external density curve from one moment when that curve has a relative minimum to the moment when it has its next relative maximum, that is, *while the derivative of the density curve is positive. It is this discontinuity of the function that makes consideration of the hydraulic theory so distasteful to physicists,*[6] that they rather, like the proverbial ostrich, put their heads where they cannot see the hydraulic theory and can comfortably dream of *resonance*, a theory which we can teach any college freshman to tackle. But Nature's realities will not let themselves be cheated out of existence by our human laziness. Of

[6]Biologists, in general, have a still further reaching attitude towards mathematical functions. That, I believe, is the reason why nearly all of them have refused to read any of my publications during thirty years. Compare footnotes 2, 8, 13, 14, 15, and 17.

course, as soon as the derivative becomes *negative*, Figures 4 and 5 have to be turned upside down, and the variable l *jumps* discontinuously from its last value back to zero. One can see it in my model!

We remember that the total work done by the pressure p during the time Δt is not alone that stated in equation (3) nor alone that stated in equation (5) nor alone that stated in equation (9), but the sum of these three:

$$p\ \Delta t = p_1\ \Delta t + p_2 \Delta t + p_3 \Delta t \tag{10}$$

To save space, we write equation (11) at once in the form of integrals:

$$\begin{aligned}\int p\,dt = \int [\ (-a_1 \sin t + b_1 \cos t) + 2\ (-a_2 \sin 2t + \\ + b_2 \cos 2t) + \ .\ .\ .\ ]dt = \end{aligned}$$

$$= 2\eta \int \int_0^l v\ D\ dl\ dt +$$

$$+K \int \int_0^l \frac{\sqrt{D}\ \ dl}{\operatorname{cotg} \frac{1}{2K}\sqrt{D} - \cos \frac{1}{2K}\sqrt{D}}\ dt +$$

$$+2\ S \int \left[D\ v\ dv + \tfrac{1}{2} \int_0^l d_p\ D\ v\ dv \right] dt \tag{11}$$

Let it be understood, then, that equation (11) is *valid only while p does not change its sign*;[7] and that the constant a_o has disappeared from the Fourier series because p is the average *slope* of the density curve of the external air over a very small distance. If you want to see experimentally how l jumps back to its zero point at the moment when that slope changes its algebraic sign, you can see it in any model constructed like Figure 6. The discontinuity of the function (11) is there obvious.[8]

[7]Compare, however, a further-going restriction on a later page!

[8]Compare this working model with the older one mentioned in footnote 15. J. P. Minton, a younger physicist with the support or more likely at the suggestion of an authority in a biological branch, J. G. Wilson, says in *Phys. Rev.*, 19, 1922, page 93, line 11: "Other theories of audition will not be referred to here, inasmuch as they have not survived the criticism brought against them." This report of the demise of the hydraulic theory seems to be exaggerated. Inasmuch as during thirty years the present writer has been actively interested in the survival of the hydraulic theory and has watched in vain the scientific publications of the world for the appearance of any criticism against it, he and everybody else ought to be indebted to Messrs. Minton and Wilson if they would kindly announce where such criticism has been published and what it consists in.

Why the time integrals of equation (11) have been left *indefinite*, is clear when we understand that the time interval during which the equation is valid, has to be computed for each new change of sign of p.

From "A Course in Mathematical Analysis" by Goursat-Hedrick, 1904, p. 3, I quote the following homely remark: "It seems superfluous to explain the reasons which lead us to assume that functions which are defined by physical conditions are, at least in general, continuous." A look at my hydraulic model teaches the wisdom of the qualification "at least in general." Our chances of finding *discontinuous functions* may be greater in living (grown) than in inorganic nature. The model's phragma imitates as closely as possible the physical properties of the living phragma. It is a piece of unstretched oil-tanned skin.

The pressure *supply* is inconstant. In the cochlea it is a Fourier series with, possibly, very numerous maxima and minima, and "grouped inflection points" thrown in for good measure, within its period. The "grouped inflection points (that is, inflection points unseparated by a maximum or minimum) are *a highly important matter for audition*, as I have pointed out twenty years ago.[9] We shall return to them on a later page. Happy, of course, are those who find their dreamy satisfaction in attributing to the cochlea the continuous function of resonance. They need not worry about such devilish things as the inflection points of a Fourier series! And whether they are grouped or not!

As stated before, our aim is to establish, for particular conditions of the constants contained in p, definite relations between l and t. It is, of course, no easy matter to draw conclusions from such an equation as that of (11). But is not *a complicated truth* better than a simple contradiction to *anatomical facts*? The unpleasant observation that the equation is difficult to solve, no more *disproves* that under the microscope we see only *"anchored"* (unstretched) strings in the cochlea, than the unsolved "problem of three bodies" *proves* that we can see only two in the planetary system. At least we shall avoid the terrific waste of intellectual energy involved in making computations on the basis of "imaginary" assumptions. The word in quotation marks is thus qualified because imaginary in the sense of "hypothetical" is unobjectionable, yea, indispensable, in science. At the time of Helmholtz it was permissible to "imagine a harp in the ear." In 1927 the assumption of "strings under permanent tension" in the cochlea is imaginary in the bad sense of "contrary

[9] "An Introduction to the Mechanics of the Inner Ear," University of Missouri Studies, 1907, pp. 85, 89, etc.

to the anatomical facts"; and computations based upon that assumption are therefore sheer waste.[10]

The next step we take consists in solving the equation for special cases. We have to distinguish two kinds of special cases. First, those in which we arbitrarily *reduce the Fourier series to a very few of its members.* Second, those in which we arbitrarily disregard some of *the more delicate features* of the hydraulic function and therefore retain within the equation only those parts which describe *the grosser features.*

The *experimental* study of a hydraulic model like that shown in Figure 6 aids us in reasonably distinguishing between *finer and grosser features of the hydraulic function.* The transparent part of the model has a length of 60 cm. It is easy to exchange the fluid for one of different gravity (S) or of different viscosity (η). It is not difficult to exchange the phragma for one of different physical properties (K). Compare equation (11) for the rôle played by these physical constants. The variations of external pressure are produced by means of a mercury level of variable height, in Figure 6 shown under hand control. What we can see from a distance with the naked eye, we regard as a grosser feature of the function.[11] What we can see only with magnifying instruments, we regard as finer features.

A very great simplification of the hydraulic equation results from substituting for the function of equation (1) a constant. We have prepared the reader for this emergency by introducing in Figure 3 the term "parathesis" as representing that linear dimension of D which depends on p rather than on l, and whose sign and magnitude depend on the sign rather than on the magnitude of p. *In the hydraulic model constancy of D can be realized.* In order to make D *independent* of l, we make the phragma (which consists of a ribbon of very soft chamois skin) equally wide all along the length of the

[10]A typical example is that of Wegel and Lane, *Phys. Rev.*, 23, 1924, pp. 276ff. I believe that I have a just complaint against these authors who quote Alfred M. Mayer as far back as 1876, for not mentioning me at all and unqualifiedly saying on page 266: "In past work on audition very little attention has been given to the phenomenon of masking." *Quite on the contrary* I have greatly emphasized its importance and published both theoretical and experimental statements about the fact on numerous occasions. I have made it *one of the cornerstones of the hydraulic theory.* To do me justice, compare the summary remark by Joseph Peterson, *Psychol. Monog.*, 39, 1908, page 93, line 16: "It is to be noted that (Max F.) Meyer's theory affords an easy explanation of the fact, which probably no other existing merely physiological theory can explain; viz., that a high tone may be obliterated by a lower one, whereas the opposite is not true." Is ignorance of the scientific literature a better excuse in physics than ignorance of the statute law is in civics?

[11]A moving picture film of the photographed gross function of the inner part of the model can be bought from the writer's laboratory at a moderate price.

tube. By hanging the ribbon "flabbily" we make its translation *independent* of the magnitude (though still dependent on the algebraic sign) of p, as long as p is held within certain bounds. D is then, *within the bounds* of the "parathesis," a constant; and the value of the phragma's resistance to pressure is then virtually plus or minus zero, but virtually infinite as soon as the parathesis is reached.

A glance at equation (7) informs us that $\Delta_p D$ becomes then zero. It is further evident that the time integral $\int v\,dv$ must be zero if we start with the whole fluid column in a state of rest, and end with it in a state of rest, as of course we shall. Therefore p_3 in equation (9) *is reduced to zero.*

A mere corollary of making D a constant, is making y in equation (2) infinitely small as long as the translation x of the phragma from its thesis remains within the bounds of its parathesis (equivalent to a constant D), and making y absolutely infinite as soon as the parathesis is reached, so that the phragma is *incapable of acquiring potential energy,* a condition which can be virtually realized in the hydraulic model. If this is the significance of equation (2), p_2 *also becomes negligible,* like p_3. But of course, nobody can expect now that the facts of audition *should agree more than approximately* with the torso that is left of the theory. The important thing to observe is the fact that *the experimental observations in audition never run contrary* to the conclusions derived from even the simplified mathematical function, that is, the right side of equation (11) reduced to the first of its three summands and then depending only on η, no longer on either S or K.

A glance at our manner of obtaining equation (3) shows that, when D is an absolute constant, *the total mass involved in the internal friction* from t_1 to t_2 (defined with reference to a minimum and a maximum of $\int p\,dt$) is proportional to the total l. Equation (11) then reduces itself to:

$$\int_{t_1}^{t_2} p\,dt = 2\eta D \int_{t_1}^{t_2} l\,\frac{\delta l}{\delta t}\,dt \qquad (12)$$

This means that the total density difference in the external air from t_1 to t_2 is for a constant viscosity and a constant D proportional to l^2. Or l is proportional to $\sqrt{\int p\,dt}$.

I want to emphasize the last statement particularly because in my former publications I have not done so, but *in graphic solutions* of the hydraulic problem have made l simply equal to $\int p\,dt$, the density difference in the external air from t_1 to t_2, a procedure which enormously simplifies the *graphic* method. But that means a total neglect of the pressure loss due to the viscosity of the fluid. The theory of audition agrees still better with the facts of audition, that is, with the *heard relative intensities* of several tones *heard simulta-*

neously, when l is justly made proportional to $\sqrt{\int p\,dt}$ and, further the anatomical D is properly taken into account, and the graphic method is thereby corrected. This is a warning for those who intend to read my former publications.[12] One must not identify my graphs with my theory. My graphs are only a convenient method of solving the problem by approximation and must be recognized as such and no more.

Let us now make an application to a particular case of the Fourier series. We regard all its constants as zero except a_2 and a_3, and these we regard as equal and negative, for an incidental convenience which will appear presently in the reference to an earlier publication. We therefore write $f(t)$ in the following form, reduced from its general form in which it appears above between equations (9) and (10).

$$f(t) = -\cos 2t - \cos 3t$$
$$p = f'(t) = 2\sin 2t + 3\sin 3t$$

We could now try to realize this case experimentally, which, on account of the perfectly definite phase relation, is not easy. Theoretically we could compute for the period of p those six values of t which limit the six intervals, within each of which separately equation (11) is valid. We could then compute and tabulate the six values of $l = \sqrt{\int p\,dt}$. We could then reason out, easily, how many qualitatively different chemical processes must result and, less easily, how intense each must be, very relatively.[13] We could arrive at

[12] *A complete list* of those, more than a dozen, which are concerned with physiological acoustics is given in the appendix to this article, because the reader would otherwise have difficulty in obtaining it.

[13] The significant idea that "loudness" depends (not necessarily in every phase of animal audition, but in normal mammalian audition) *on the number of sensitive cells stimulated,* that is, on the variable l in our equations, is nowhere found in literature until I printed it in 1896. When, as a young student, having recognized this idea as a mathematical consequence of the hydraulic function, I first pronounced it before my worthy professors, Carl Stumpf and Max Planck, I did so with fear and trembling. They did not ridicule it, however. They admitted its plausibility, though novelty. How surprised, therefore, I was to read in an article by G. W. Wilkinson (*Amer. J. Psychol.*, 38, 1927, p. 258) discussing another article published in 1926 by E. G. Boring, the following words which ascribe this discovery to Boring and make my own views appear idiotic: "His (Boring's) theory has a resemblance to that of Meyer, — — the difference being that he (Boring) correlates the linear dimensions of the 'bulge' with intensity, and not with the duration of a single wave impulse, as does the latter (Meyer)." Do I do that? It is obvious that Wilkinson is one of the numerous biologists who, knowing of my views only by hearsay, nevertheless feel competent to (mis)represent them and to fasten on them the scarlet letter. Boring called my theory "famous" and thus scarcely gave him an excuse for misbranding it. *How can a young graduate student, nowadays, find correct information in going through the mess of distorted and deceitful literature?* Let this footnote stand as a warning to the student, not to believe everything he reads.

the intensities of these chemical processes still more exactly if we assumed and realized experimentally the factors (amplitudes) in the Fourier series $f(t)$ which we omitted above, or, rather, which we took as *minus one*. We could try to solve the anatomical problems of knowing the constants, S, η, and K and the exact dependence of D on l. Will anybody, anatomist or physicist, rise and proclaim that the hydraulic theory is the wrong theory *because we do not do all that this very day?* Better have faith in that fanciful harp?

We fall provisionally back upon graphic methods. Many years ago I invented *three* graphic methods, all quite different from the draftsman's point of view and yet serving exactly the same purpose.[14] They are still good within the limits of their purpose. Applying now any one of these methods to our case, we find that our theory calls for the hearing of three tones although this Fourier series contains only two elements. Take two *tuning forks* and convince yourself that the prediction of the theory is correct. In order to keep any wiseacres from exclaiming, "Oh, yes, but that has a different reason," I advise them to read my article on "Asymmetry-tones" in the *Annalen der Physik*, **12**, (pp. 889-892, 1903,) which is still true. It is not my intention, however, to reprint in the present article what *I have published* elsewhere in numerous publications, and *for a great variety of cases,* not merely for the simple one above. Let me only mention that the very simple case under discussion in the last two paragraphs is *graphically* illustrated, for example, in *Pflueger's Archiv*, **81**, pp. 64 and 68.

The ear is not a harmonic analyzer: Induced and Masked Tones. This we must learn, however hard it goes into brains crammed full with prejudices. It is very convenient indeed to represent $f(t)$ *always as a Fourier series* of a larger or smaller number of finite elements, since sounds nearly always (or rather, more frequently than not) *have their origin* in elastic bodies or in other physical phenomena to which the principles of resonance apply. But the cochlea's hydraulic analysis of its load into frequencies *is not a harmonic analysis and may yield often fewer and often more motion frequencies* of separate phragma lengths than the number of trigonometric elements in the

[14]These three very useful graphic methods were first described, successively, in the following three publications: I. *Zeitschrift für Psych. u. Physiologie der Sinnesorgane*, **11**, 1896, pp. 218-219. II. *Zeitschrift für Psych. u. Physiologie der Sinnesorgane*, **16**, 1898, pp. 26-27. III. *Pflueger's Archiv. für die gesamte Physiologie*, **81**, 1900, p. 68. — —Unfortunately biologists, in general, do not seem to like graphs any better than mathematical analysis. They have never offered any criticism of these graphic methods, their purposes and their limitations. The total lack of such criticism has been a greater source of disappointment to me than a proof of the worthlessness of my fundamental conceptions could have been.

Fourier series describing the air density.[15] That depends on the variety of conditions which stare us in the face in equation (11). When we hear *more*, I teach my students to speak of *induced* tones, a term which biologists ought to like because it is analogous to induced colors. When we hear *fewer*, we might with Wegel and Lane speak of tones having been *masked*. Inducing and masking thus refer respectively to the excess over and the remainder of the frequencies indicated in the Fourier series. The introduction of such terms into our scientific language is highly desirable.

A "vivisectional" test of the hydraulic theory. It is easy to show that on account of the *rather high viscosity* the length of the phragma piece involved, which depends mainly on the difference between two neighboring maximum and minimum values of $f(t)$, is by no means independent of *the average slope* of $f(t)$ *between these limits.* Instead of average slope we can say "frequency," using the quotation marks as a reminder of the fact that we refer to the "shakes" of the phragma and not to the arguments within the Fourier series, which are frequencies in a different, the ordinary, sense.

Let us then consider a constant D and a fixed time interval from a moment when p is zero to the next moment when it is zero, calling these moments t_1 and t_2, as we have previously done. A glance at either equation (12) or equation (3) teaches that *for an average push* p_a *over the fixed time interval* which concerns us, l *is small when* v *is great, and reversely,* v *is small when* l *is great,* for

$$p_a = 2\eta D\, lv \qquad (13)$$

If η were a factor of small influence in equation (11), the result of equation (13) would not mean much. But the *viscosity of the cochlea fluid is not small,* for the fluid is not pure water, but a colloidal substance like liquid glue.

The time derivative of l, that is, v, has of course virtually a certain proportionality to p_a, the average push. A moment's thought devoted to equation (13) then makes clear that the variable l, *the length* of the phragma affected, *more or less lags behind the magnitude of the driving force* p_a, or, in other words, roughly speaking, the total length of the phragma piece involved has a better chance to approach proportionality to the driving force, if the frequency of the air density curve $f(t)$ remains small. (Always remember that p is the slope of $f(t)$!) Or, in still other words, *the higher a physical tone* (constant air density differences, i. e., amplitudes, taken for granted with different pitches) *the smaller its "sphere of influence"*

[15] If neither graphs nor analysis convince, my recent model might do it. But thirty years of disappointment have made me pessimistic. Did I not publish in a physiological magazine the description of a simple, but useful, *hydraulic* model as long ago as 1899? With no effect!

measured from the cochlea windows. It is quite easy to demonstrate this to unbelievers on a hydraulic model.

Such facts enable us to make a kind of "vivisectional" test of the hydraulic theory on the cochlea of living animals. For this purpose we must remember that *the width of the phragma* is by no means constant, but increases from the windows for at least two-thirds of its length (and then decreases again, to tell the truth). *With its width increases its translation.* An engineering course can teach us that in such a thick and spongy substance as the phragma *the mutual shearing and mutilation* of its members is greatest where the translation is greatest. Cell degeneration as well as formation of scar tissue is to be expected, therefore, *toward the end of a tone's "sphere of influence"* rather than nearer the windows, without forgetting that we refer to physical tones varying in pitch but constant in air density differences.

The test, which requires great patience as well as the equipment of a physiological and a histological laboratory, was made long ago by a skilful Japanese investigator, Yoshii,[16] who subjected numerous guinea pigs to tones of *different pitch* produced by means of a *constant air blast,* for three months, without giving the animals a chance to hide. What the hydraulic theory predicts, was found to be true. Degeneration of the more delicate cells and formation of scar tissue were found in regions of the phragma varying in distance from the windows: *The lower the tones, the farther the scars. The higher the tones, the nearer the scars.* What proof could be more beautiful!

There will, of course, be those who raise their voices in order to urge that Yoshii's results may also be explained *by believing* that high-tone resonators have been located by the divine harp-builder near the windows, and low-tone resonators farther away from the windows. One can indeed believe lots of things if *credulity is elevated to the position of a virtue.* But I do not hesitate publicly to classify those who believe such things, together with that part of the human population which believes that the stork brings the babies.

The vast import of grouped inflection points of the air density curve. If we limit ourselves to the *grosser* features of the hydraulic function, for whose description we derived equation (12), a mere torso of the true mathematical function, we must not be surprised when the theory predicts a great deal more "masking" of higher tones by lower tones than actual experience finds to be true. I have clearly indicated this in former publications, many years ago. I do not intend to reprint all of it.

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[16] *Zsch. f. Ohrenhk.*, 58, 1909, pp. 201-251.

We cannot neglect p_2 in equation (5). As soon as the phragma has moved from one parathesis into the opposite parathesis and continues in the same direction, it *begins to acquire potential energy on its way toward the hyperthesis,* as indicated in Figure 3. It is then no longer possible to use equation (12). We return to equation (11). And it is even no longer true that equation (11) is valid in every interval in which $f'(t)$ does not change its sign. Owing to the fact that a cotangent rapidly approaches the value of a cosine when we have passed the smaller angles, say, have left behind us the first octant, equation (11) is valid only in a time interval in which the *second* derivation $f''(t)$ does not change its sign. Compare footnote 7. In every case in which the curve of $f(t)$ shows (three) grouped inflection points, $f''(t)$ changes its sign at the inflection point before the preceding, and at the one after the following, minimum or maximum of $f(t)$ and at each one of the inclosed three inflection points of $f(t)$. We would have then between the first and the fifth of these five inflection points of $f(t)$ the equivalent of *only one* "double-shake of the phragma, in accordance with the reduced equation (12), if we meant by a "single-shake" its motion from one hyperthesis to the other hyperthesis; *but we have the equivalent of two* "double-shakes of the phragma, in accordance with equation (11), if we mean by a "single-shake" the motion between opposed hypertheses and *also* the motion between a hyperthesis and its adjunct parathesis. Conclusion: the higher tone is not yet masked, as long as there remain grouped inflection points belonging to it.

All this is far from being merely "speculative."[17] It can be easily demonstrated on the hydraulic model by giving the phragma those physical properties, K of equation (11), which make it easy to observe. For example, we hang our chamois skin so flabbily that the distance from thesis to parathesis is about 6 mm. and make with the aid of a magnifying glass the following observation. A particular point L of the phragma goes into its parathesis under the

[17]The only "criticism" of the hydraulic theory I have ever found in the scientific literature consists in throwing at it the epithet "speculative." And if it were so, is mathematical speculation, with which I started over thirty years ago, "untrue" as such, that is, because it is speculation? Was not the planet Neptune discovered by speculation? Mathematical speculation becomes vicious only when, on *doubtful premises,* it nevertheless insists on predicting that *which the eye has already discovered to be untrue,* e. g., on hypothesizing "stretched" strings in the cochlea, and on letting "faith" *deny the testimony of the senses and backwards demonstrate those doubted premises.* For an example of the latter procedure see Wilkinson and Gray, *The Mechanism of the Cochlea,* 1924, who (p. 82) faithfully assert that a *brass* harp with *phosphor-bronze* wires stretched by *weights* and then *soldered* to the brass frame, is a "working model" of the cochlea! The *living cochlea!* Compare in footnote (20) the crushing remark made by Bonnier.

influence of a weak pressure p. We increase the pressure without changing its sign and see through a weak magnifying glass that L goes into the hyperthesis by adding about one millimeter to its distance from the thesis. We diminish the pressure without changing its sign and observe that L goes back from 7 mm. into the adjunct parathesis at 6 mm. distance from the thesis. We increase the pressure (this means $f'(t)$, of course) without changing the sign, and L goes again into the hyperthesis. Now we change the sign of the pressure p, and L goes into the opposite parathesis. Etc., etc. In my laboratory you can convince yourself. Still better, construct your own hydraulic model. I shall be glad to give to anybody who intends to make one up the specifications of my own. Count on spending two hundred dollars.

There is no reason why we should refuse to admit that a double motion ("double-shake") between parathesis and adjunct hyperthesis has *the same chemical consequences in the sensitive cell* as a double motion between opposite hypertheses.

Why are changes in the phase relations of simultaneous tones so unimpressive? That shifting the phase of a tone relative to another *can never have a very conspicuous audible effect* is clear to anyone who has taken the time to make *the hydraulic theory* clear to himself. Pierre Bonnier deserves much credit unjustly withheld from him in the physiological textbooks, for having been the first to return to the hydraulic notions prevalent at the time of Johannes Müller and to suggest the replacement of the resonance theory by a hydraulic theory. But he has caused an almost unpardonable confusion by speaking rather vaguely of the phragma as if it were a kind of gramophone plate on which music could be "recorded."[18] But he fails to give an exact and mathematical description, either graphic or analytic. That gives to loose thinkers, of whom there are plenty, the idea that "the profile" of "the record" must be something im-

[18] *J. de Phys.*, 5, 1906, p. 591: "La papille enregistre ainsi les moindres détails de cette variation de pression qui est l'ébranlement sonore, les intensités se mesurant par la force du tiraillement; c'est une opération d'ordre tactile; les periodicités et les formes de l'ébranlement, si complex soit-il, s'enregistrent comme sur une plaque sensibilisée. — —L'oreille est ainsi comparable aux enregistreurs barométriques et manométriques que nous connaissons. La plaque sur laquelle l'ébranlement sonore s'inscrit est une merveilleuse papille, délicatement suspendue, sur laquelle la courbe ondulatoire promène elle-même les divers détails de sa forme. — —Je n'ai voulu ici marquer que le mécanisme de l'oreille *enregistreur*. — — En résumé, l'oreille est un enregistreur, fonctionnant selon le type de nos enregistreurs barométriques. L'ébranlement laisse son empreinte sur la papille du limaçon d'une façon continue, chaque point de la courbe ondulatoire s'inscrivant successivement sur tous les points de la papille auditive par un procédé purement tactile." The word "enregistreur," i. e., *recorder*, is emphasized in Bonnier's original text. The cochlea a *recorder*?

portant for audition,[19] whereas actually, in theory and practice, "the visible profile" of the curve is of no consequence at all, the same hydraulic function corresponding to *enormously varying profiles.*

The phragma ("la papille" of Bonnier), *contrary to Bonnier's statements, in no way resembles a gramophone record.* The fact of induced and masked tones ought to make this negative statement decisive. The acoustic-analytical mode of thinking unfortunately is entirely foreign to Bonnier. The result is this misconception by a "morphologist" too strongly inclined to conceive "form" only in the concrete.

As soon as we become aware of how much depends on whether $f''(t)$ changes its sign or not, we can discover a method which gives us a maximum of experimental chance by mere listening to become aware of an artificially produced change of phrase relations between two (or more) simultaneous tones. As we said above, masking is greatly diminished in accordance with the fact that the members of the trigonometric series remain audible even though in the curve of $f(t)$ they make themselves apparent only by a group of inflection points and not by a relative extremum of the curve.

We might see if there is a condition under which, by a mere change of phase, a tone previously weakly audible is "just" masked.

Everybody will admit as a psychological principle that it is easier to become aware of the difference between being *now weak and then completely but barely gone* than of the difference between being now weak and then *a trifle more or less weak.* Of course, there is no chance, in accordance with the Weber-Fechner law, of ever becoming aware of the difference between now strong and then a trifle more or less strong.

Remembering that it all depends on whether $f''(t)$ changes its sign more or less frequently, we recognize that the realization of *that condition which most favors* observation by a mere listener, for which we are looking, depends on fulfilling two conditions for $f(t)$.

I. We must have two sinusoidal waves whose *amplitudes are* (because they are under the influence of the *second* derivation) each *exactly proportional to the square* of the numerical factor (p or q) of the other argument within $f(t)$.

[19]This is asserted even by Bonnier, who in most other respects (especially as a morphologist) is so clear-visioned but who has steadfastly refused to pay any attention to my, obviously "too mathematical" work. Compare *J. de Phys.*, 5, 1906, p. 583: "Le timbre est lié à la forme de l'ébranlement." And p. 582: "Nos *centres nerveux savent,* surtout s'ils sont éduqués, *spéculer* sur l'apport sensoriel. — —Cette analyse n'est pas sensorielle, elle est *centrale et spéculative.*" In none of Meyer's publications will you find any "speculative" doctrine like that. I teach my students that *nerve centers are physico-chemical conductive agents.* I live in the twentieth century and not in the epoch of Aristotle's entelechy. Our nerve centers "can speculate"? Is that physico-chemical science? Is that a theory of audition?

$$f(t) = q^2 \sin px \pm p^2 \sin qx$$

$$\frac{1}{pq} f'(t) = q \cos px \pm p \cos qx \qquad (14)$$

$$-\frac{1}{p^2q^2} f''(t) = \sin px \pm \sin qx$$

II. The *phase* relation for the case in which we want the masking *just* to occur must be exactly that determined by equation (14); and the phase *shift* for the comparative unmasked case must be determined exactly by equation (15).

$$f(t) = q^2 \sin p\left(x + \tfrac{1}{2}\pi \frac{q-p}{pq}\right) \pm p^2 \sin qx \qquad (15)$$

I hope sometime experimentally to realize this double condition in which *one might possibly observe the faint effect* of the phase shifting. If anybody else should beat me to it, I shall feel no jealousy. In the music hall, of course, this condition will never accidentally be realized in a practical way for the audience; and the hydraulic theory, therefore, frees the composer, when he writes his orchestration, from all worry about a possible change in the sounding of his music at a repeated performance.

What has always astonished me is the fact, that the scientific literature, textbooks of physiology and, still sadder to relate, textbooks of physics included, is full of such dogmatic statements as this: that the practical irrelevancy of phase shifting in music *can not be explained* except "by faith in the existence of a harp in the ear," changing the biblical admonition "Prove all things; hold fast that which is good," into the version, "Reject blindly everything except what is incredible." The harp is an incredible fantasm of animal morphology.[20]

An interesting relation between pitch and intensity. When I first became concerned, more than thirty years ago, with the need of developing a theory of audition *based essentially on non-elasticity of the cochlea elements,* I noticed at once that this would require a totally new conception of the physiological basis of sensation intensity so far as the sense of hearing was in question. As stated above, the hydraulic theory makes *sensation intensity dependent on the number*

[20]Bonnier, *J. de Phys.*, 5, 1906, p. 580: "Un résonateur ne peut varier sans se fausser, et *un résonateur vivant ne peut que varier incessamment.*" But nobody listened to Bonnier any more than to me. And the whale swallowed Jonah, or rather the harp.

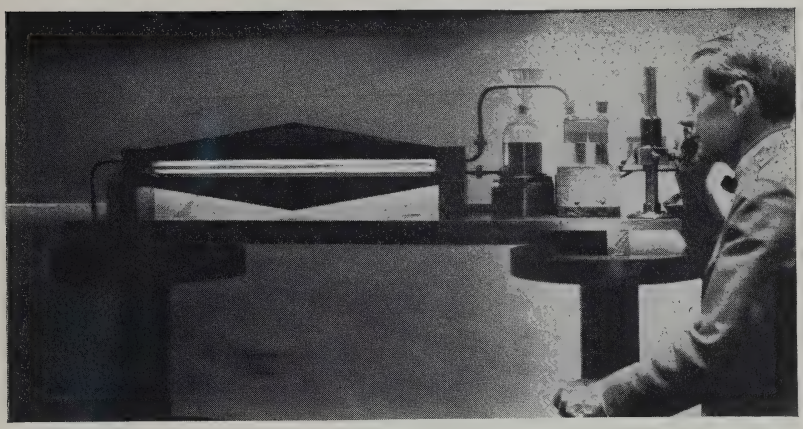

FIGURE 6

A Hydraulic Model

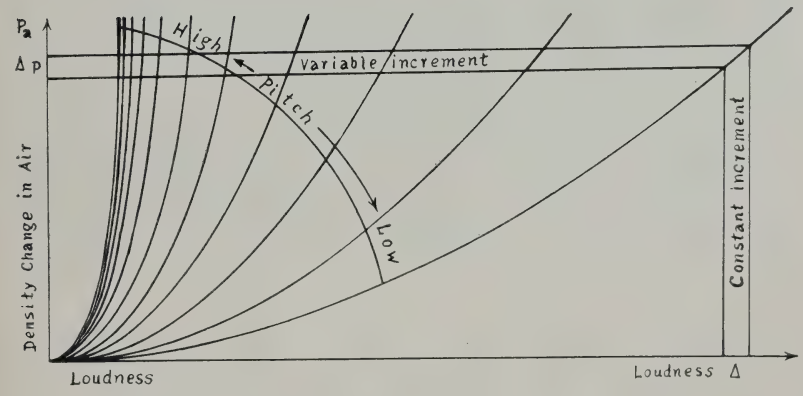

FIGURE 7

A Family of Parabolic Curves

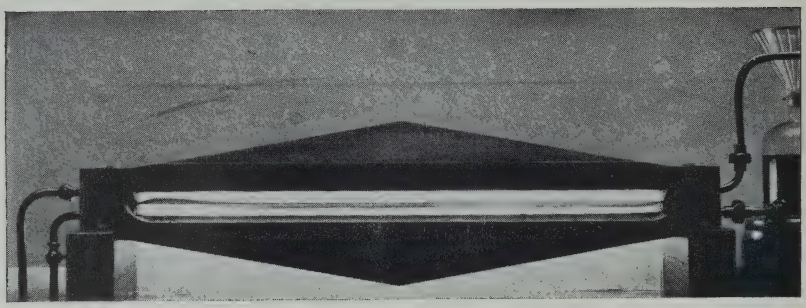

FIGURE 8

THE PHRAGMA IN A FORTUITOUS POSITION

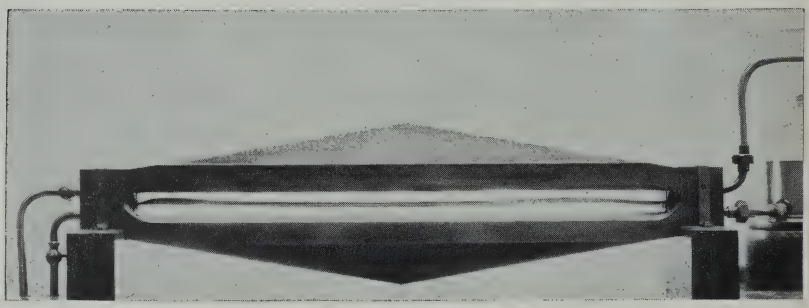

FIGURE 9

THE PHRAGMA AT A LATER MOMENT THAN IN FIG. 8

Notice the changes which have occurred on the right; none on the left.

of sensitive cells stimulated, at least in what we may call the "normal" functioning of the organ.[21]

We stated above that, due to the viscosity of the cochlea fluid, l must the more lag behind p, that is, behind an increasing density difference in the external air, the greater the parameter of frequency. The consequence of this fact for sensation intensity must still be intensified by the fact that D is not independent of l. The result must be a kind of "family of parabolic curves" describing the growth of sensation intensity for varying pitch in the manner of Figure 7. No attempt has been made in the figure to space the parabolic curves in accordance with any *determined* mathematical function of varying wave-length (or frequency, pitch of tones).

We now cut Figure 7 by a narrow horizontal ribbon which represents a *variable increment* of p_a that is, of the average slope of $f(t)$ between two neighboring extrema of the curve of $f(t)$. This Δp we refer to as a *constant loudness increment*, represented by the vertical ribbon of Figure 7. We then see clearly that, if we plot for a constant loudness increment this Δp against the frequencies of the figure, we must expect a peculiar new curve to result. As we pass from smaller to greater frequencies, this new curve must first rise rapidly, but must later rise more and more slowly.

This matter has recently been subjected by B. A. Kingsbury to a systematic experimental test, which confirms the predictions of the hydraulic theory. Of course, such predictions refer only to the middle regions of audition. In order to predict for either of the extremes of frequency, one would have to know and take into account *all the anatomical* facts, not to mention the *chemical* (virtually unknown) facts on which depend the upper and lower pitch limits of audition. The curve in question is Kingsbury's[22] Figure 4, describing the "Effect of frequency upon T. U. increment between loudness contours." *This effect is unrelated to any physiological theory of audition other than the hydraulic theory.*

Kingsbury attempts somehow to bring his remarkable experimental results into relation with Weber's law. All respect for Weber's law—but when Kingsbury's data are brought also into relation with *the hydraulic functions* of the cochlea, his experiments assume an importance which the author did not foresee or even suspect. And why is it unfashionable among physicists to consider *the hydraulic functions* of the cochlea? Because our physicists have not emancipated themselves from the authoritative assertions of physiological text-book writers, who, incapable of comprehending mechanical functions

[21] Compare in my text-book, The Psychology of the Other-One, pp. 294-299, the discussion of the three (more primitive and more evolved) modes of functioning, not mutually exclusive at the same time.

[22] *Phys. Rev.*, 29, 1927, p. 592.

that go beyond the law of the lever, have chosen to fall back upon that fairy tale of "the harp in the ear" and—here is the thing to marvel at—quote in support of their fairy tale nothing but the physicists' modest willingness to subject themselves to those textbooks in anything that concerns the living body.

The concept of a traveling bulge is not a part of the hydraulic theory. The physics student is likely to be deceived into a wrong conception of the hydraulic theory by the fact that not a few biologists refer to it as the "traveling" bulge theory.[23] A bulge indeed forms at the point l=zero, but it does not travel; the bulge merely lengthens, that is, lengthwise increases on the side of the phragma, above or below, where it has formed, until the moment of discontinuity of the mathematical function. Then the bulge originates at the point l = zero on the other side of the phragma, lengthens until the new moment of discontinuity, etc., etc. Figures 8 and 9 illustrate this.

Figure 8 represents an earlier, Figure 9 a later phase within the same compound period which corresponds to the limits of t in the Fourier series. He who compares the picture of the phragma at the later moment with that at the former, notices on the left that the remnants of the two bulges first created, the first above the thesis, the second below, in the meantime have not traveled at all. They have simply stood still. (In my model they may be seen to stand like that, unchanged, for days.) Of course, that is all one could expect, since between taking the two photos no external forces have extended their effects so far to the left, and since, having stayed within the parathesis, the phragma in that left region has virtually no acquired potential energy. This fact of "standing still" is exactly what I have taught consistently for thirty years. A permanent tension of the phragma, because of its biologic-anatomical contradictions, I have steadfastly rejected. And *without tension*, we know as physicists, *no traveling* of any bulge is possible.

[23]One of the more recent examples is Wilkinson and Gray, The Mechanism of the Cochlea, p. 118, l. 21: "Meyer . . . assumes . . . . tension of the basilar membrane . . . etc." What I really have assumed constantly since my first going into print in 1896 is reproduced in equation (2) and in Figure 3. And on page 214, line 6, of the same book: "In Meyer's theory . . . a bulge travels onward". Never and nowhere have I sponsored the concept of a "traveling bulge." I do not understand why this bastard conception travels under my name, spoiling my scientific reputation.

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THE EFFECT OF AN UNEXPECTED HETEROGENEITY ON ATTENTION*

From the Psychological Laboratory of the Uniwersiteit van Stellenbosch, Suidafrika

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A considerable number of writers have expressed, and more or less fully developed, the view that what is heterogeneous, in some respect or other, with the simultaneous or the more immediately foregoing contents of consciousness, with previous experience in general, or with expectation, attracts or holds attention. A number of the contributions on the subject will come in for discussion in the present article. We are, however, more particularly concerned in it with views presented by Selz on the phenomena in question, and shall therefore begin by giving his statement of them.

Selz formulates as *das Gesetz der Widerstreitsbeachtung* that ". . . ein Erlebnis, dass an Stelle eines erwarteten Erlebnisses eintritt, die Tendenz hat, ohne eine besondere darauf gerichtete Determination zugleich mit dem bestehenden Widerstreit bemerkt zu werden."[1]

Selz further formulates a *Gesetz der Distinktionsbeachtung* according to which "Stimmt die Überwiegende Mehrzahl der Bestandstücke eines Erlebniskomplexes in irgend einer Hinsicht überein, so hat ein demselben Komplex angehörendes Bestandstück, das in dieser Hinsicht von ihm abweicht, die Tendenz, beachtet zu werden."[2]

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[1]O. Selz, *Zur Psychologie des Produktiven Denkens u. d. Irrtums,* 1922, 217. Under this law Selz subsumes the frequent occurrence of negative judgments with regard to solutions obtained by processes of thought directed by an *Aufgabe.* He holds that, "Das Bemerken des Widerstreits zwischen einem antizipierten festzustellenden Sachverhältnis und dem tatsächlich bestehenden Sachverhältnis darf als eine psychologische Hauptquelle des negativen Urteils angesehen werden," and that, "Die auf die Abstraktion der Übereinstimmung gerichtete determinierende Tendenz genügt . . . um die Abstraktion der Nicht-übereinstimmung des einen Sachverhaltgliedes mit dem Reizwortgegenstand oder der tatsächlich bestehenden Beziehung mit der Aufgabebeziehung zustande zu bringen" since "Das bestehen einer . . . . determinierenden Tendenz zur Feststellung eines allenfalsigen Widerstreits ist möglich, aber wohl nicht erforderlich . . . Der Zustand der Enttäuschung gründet sich auf eine auf diese Weise herbeigeführte Beachtung, ebenso die Tatsche, dass die Veränderung einer gewohnten Erscheinung die Aufmerksamkeit auf sich zieht." *Über die Gesetze des geordneten Denkverlaufs,* 1913, 275-6.

[2]Selz further states:—"Wie die Reproduktionsbereitschaft, so ist auch die Abstraktionsbereitschaft abhängig von der augenblicklichen psychischen Gesamtverfassung, wie sie durch die Gesamtheit der gegenwärtig vor-

The present article gives an account of a series of experiments carried out by the writer with the aim (1) of attempting a further demonstration, under laboratory conditions, of the phenomena in question, and (2) of obtaining the data needed for determining possible limitations of the validity of the two laws formulated by Selz for them.

At the outset two broad types of cases have to be distinguished. These are: (1) An occurrence, while showing some degree of heterogeneity with preceding contents of consciousness, was, still, expected by us at some time or other. Such expectation then forms a condition favourable to the content attaining a higher degree of attention when it does occur.[3] Even a very general *Einstellung* for the new

handenen Bedingungen psychischen Geschehens bestimmt wird, also durch die psychische Konstellation im weitesten Sinne des Wortes. So wird die Abstraktionsbereitschaft eines Bestandstücks dadurch gesteigert, dass es sich von der Gesamtheit der Bestandstücke des simultanen oder suksessiven Erlebniskomplexes oder bei determinierter Abstraktion von der Gesamtheit der Bestandstücke der Abstraktionsgrundlage in irgend einer Hinsicht unterscheidet. Darum fällt im Zusammenhang des Gewohnten das Neue, im Zusammenhang des Ungewohnten aber das Bekannte auf. Ebenso im Zusammenhang von Empfindungen mittlerer Intensität eine sehr intensive Empfindung, im Zusammenhang sehr intensiver Empfindungen dagegen eine sehr wenig intensive, z. B. nach einer Fortissimo ein einsetzendes Pianissimo. Im Zusammenhang des Kleinen fällt das Grosse, im Zusammenhang des Kleinen fällt das Grosse, im Zusammenhang des Grossen aber das Kleine auf, im Zusammenhang des Ruhenden das Bewegte, im Zusammenhang des Bewegten aber das Ruhende, im Zusammenhang des Gleichen das Ungleiche, dagegen im Zusammenhang des Ungleichen das Gleiche . . . . Die hohe Abstraktionsbereitschaft ('Auffäuligkeit') der Stille ist in erster Linie auf das Gesetz der Distinktionsbeachtung zurückzuführen. Sie lässt sich nur aus der psychischen Gesammtverfassung verständlich machen, die durch das augenblickliche Vorhergehen oder das Gewohnheitsmässige Vorhandensein von Geräuschen bedingt ist. Die Stille, wenigstens das, was sich in ihr Beachtung aufdrängt, ist ein Sachverhältnis und zwar ein negatives Sachverhältnis . . . ." pointing out, as against Dürr, that "nicht nur die Veränderung . . . . sondern auch die'ruhende Verschiedenheit'erzeugt unter den angegebenen Bedingungen eine Abstraktionsbereitschaft." *Zur Psychologie,* etc., 495-496.

[3]So, e. g., Henning, in experimenting with series of numbers with tachistoscopic presentation, sometimes unexpectedly introduced a letter among the numbers. In other cases, where the series consisted of letters, a number was suddenly introduced among the letters on the card. He found that "Steht ein Buchstabe an fünfter Stelle in einer sechsstelligen Zahlenreihe, so wird er im unwissentlichen Verfahren bei ungezwungenem Verhalten selten aufgefasst (und analog eine Ziffer in einer Letternreiche usf.) Erfährt die Vp. jedoch vor der Lesung, dass ein Buchstabe unter lauter Ziffern sein wird, so werden damit die Residuen der Buchstaben in Bereitschaft gesetzt, und die Aufmerksamkeit stellt sich darauf ein. Der Erfolg ist, dass die Vp. häufig den Buchstaben auffasst, allein dabei bleiben mitunter alle oder doch einige der im Wahrnehmungsbilde gesehenen Ziffern unerkannt." Henning, Versuche über Residuen, *Zsch. f. Psychol.*, 78, 1917, 235-6; cp. also 238-40.

So, too, certain effects observed by Meumann are, we hold, *in part* due to

will act in this direction, as in the case of a research worker entering on a new field and generally on the lookout for phenomena differing from those of his previous experience. (2) The heterogeneous occurrence, while still showing some degree of similarity with previous or other simultaneous contents of consciousness, takes place unexpectedly. It is this type of case which we have aimed at realizing in our experiments and of which the *primary* attentional effect will occupy us more particularly.[4]

Experiment I[5]

Since, other things being equal, the retention of, and the ability to reproduce, a content varies directly with (not, necessarily, proportionately to) the degree of attention given the content at or

the same cause. In making experiments on Memory by means of the method of recognition he introduced " . . . .die Variation der Versuchsbedingungen . . dass der Vp. mitten unter den vorher gelesenen Silben neue, in der gelesenen Reihe nicht vorkommende Silben dargeboten wurden; diese wurden in unregelmässiger Reihenfolge unter die vorher gelesenen Silben gemischt, die Vp. wusste nur, dass überhaupt unbekannte neben den vorher gelesenen Silben vorkommen würden." Meumann found that "Sobald . . . die gelesenen Silben ein gewisses Mass von Bekanntheit erlangt haben . . . . heben sich die neuen Silben scharf, wie durch unmittelbares 'Gelfühl' ihrer Neuheit, von den . . . . schon dargebotenen Silben ab . . . . die unbekannten Silben heben sich für das Bewusstsein mit erstaunlich viel grösserer Bestimmtheit ab als die halb bekannten . . . ." E. Meumann, Über Bekanntheits-und Unbekanntheitsqualität, *Arch. f. d. ges. Psychol.,* **20,** 1911, 36-8.

[4]With the proviso of the unexpected occurrence of the event, this type of case thus agrees with that for which, according to Dürr, a change draws attention. It is "die Veränderung, und zwar die Teieweise Veränderung" which is "die erste Bedingung der Aufhebung." If a change has occurred in a total content known to us previously then "Das Auffallende an ihm wird durch eine Veränderung innerhalb der Totalvorstellung von ihm merklich, wie er als Totalerscheinung merklich wird dadurch, dass das Auftauchen der Vorstellung, von ihm eine Veränderung im Ganzen des gerade vorhandenen Bewusstseinsinhaltes bedeutet . . . . Auch der 'Reiz der Neuheit' ist durch nichts anderes bedingt als durch die Abhebung ausgewechselter Bestandteile. Wäre in einem Teil der Zeit das ganze Bewusstseinsleben ausgefüllt von den Inhalten a b c d e f g . . . . und wurde zu einer anderen Zeit der Totalinhalt m n o p q r . . . die ganze Seele erfüllen, ohne dass Bestandteile des ersten Komplexes jemals mit solchen des zweiten zusammen vorhanden wären, dann würde das Neuwerden des ganzen Bewusstseinsinhaltes durchaus keine Abhebung bedingen . . . ." E. Dürr, *Die Lehre von der Aufmerksamkeit,* 1914, 44-49. W. B. Pillsbury, *Attention,* 1908, 49-50, also subsumes the attentional effect of newness itself under the generalization that change attracts attention.

[5]A preliminary account of the Experiments I to IV has been given by the present writer in the *South African Journal of Science,* **33,** 1104-1112, under the heading 'The memory effect of an unexpected heterogeneous element among a group of otherwise homogeneous elements.'

after presentation, we are able to make use of the memory effect as a measure of the degree of attention which the content has had. Experiment I (and also II, III, and IV) depends on this principle. In general, the Experiment consisted of showing the subject in succession a number of series of letters, the letters appearing singly in each series. Immediately after the exposition of each series the subject had to write down as many as possible of the letters which had been shown in that series. The letters shown a particular subject were all of the same colour throughout, with the exception of one letter in one of the different series shown him, this series occurring last in the Experiment, and the change of colour being unexpected by the subject. The letter having the heterogeneous colour in the last (critical) series we shall call the critical letter. The memory effect for this letter forms our measure of the degree of attention given the letter by the subject. The subjects were informed at the beginning of the Experiment that recall would be required, and were instructed to write down only those letters of the correctness of which they were at least reasonably certain. From the fact that extremely few letters were incorrectly indicated as having occurred, we conclude that these instructions were conscientiously carried out.

The following are the further details of the procedure. With the aim of eliminating, in the total result, disturbing differences of difficulty in the separate series, four sets of these were prepared. Each complete set consisted of ten series of twelve different letters each, only consonants being used. The letters used in one set were so chosen and placed, that no sequence or part-sequence in one series of a set was repeated in another series of that set. In set (A) all the letters were of a red colour, with the exception of a blue letter in the critical series. In set (B) all were blue, with the exception of a red letter in the critical series. In set (C) all were red, with the exception of a green letter in the critical series; and in set (D) all were green, with the exception of a red letter in the critical series. In all cases the critical letter came eighth in its series. The letters were printed by hand in bold capitals on the discs of the Ranschburg memory apparatus, and were carefully made so as to be, at least approximately, of the same size. One such complete set was used with each of the subjects taking part in the Experiment, the different sets being distributed as nearly as possibly equally among the subjects. The series in each set were always taken in the same order, the first three series of a set being used for practice purposes, and the critical series always coming last of all, as has been stated. The series were shown one at a time by means of the Ranschburg memory apparatus, each letter being shown for one second. The mercury contact metronome, by means of which the memory apparatus was actuated, was placed in an adjoining room with a view to diminishing any disturbance due to its noise. The subjects were taken singly, the Experiment being completed with each subject at one sitting lasting between half and three-quarters of an hour.

Fifty subjects took part in the Experiment, by far the greater proportion of them being untrained by any previous experimental memory work. All were University students. Not only were they unaware of the fact

TABLE 1

EXPERIMENT I

Series	Position of letters 1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	11th	12th	Total
1	32	29	29	17	15	19	12	17	22	18	26	23	259 43.2%
2	25	29	28	30	17	17	10	10	19	27	24	30	266 44.3%
3	26	25	22	21	22	17	23	12	21	22	23	29	263 43.8%
4	26	23	22	22	20	11	17	21	27	20	28	29	266 44.3%
5	30	24	17	22	19	20	19	20	24	24	26	26	271 45.2%
6	35	27	23	21	23	16	21	21	19	14	28	28	276 46 %
7 (Crit.)	26	27	27	22	14	19	12	30 60%	17	16	26	26	262 43.7%
Total	174	157	141	133	116	100	102	101	132	125	155	165	
Total excluding Crit. Series	58%	52.3%	47%	44.3%	38.7%	33.3%	34%	33.7%	44%	41.7%	51.7%	55%	

that a change of colour would occur, but they were also totally ignorant of the aim of the Experiment.[6]

Table 1 gives the results obtained from all the subjects together, the results of only the practice series being omitted. The table is drawn up on the basis of one mark for each letter correctly given, the scores being given, in addition, in terms of percentages where found desirable.

It will be seen that the totals, given in the right-hand column, for the different series are very nearly the same, the difference between any two series never being as large as 3%. Such factors as fatigue or practice effects arising in the course of the experiments,

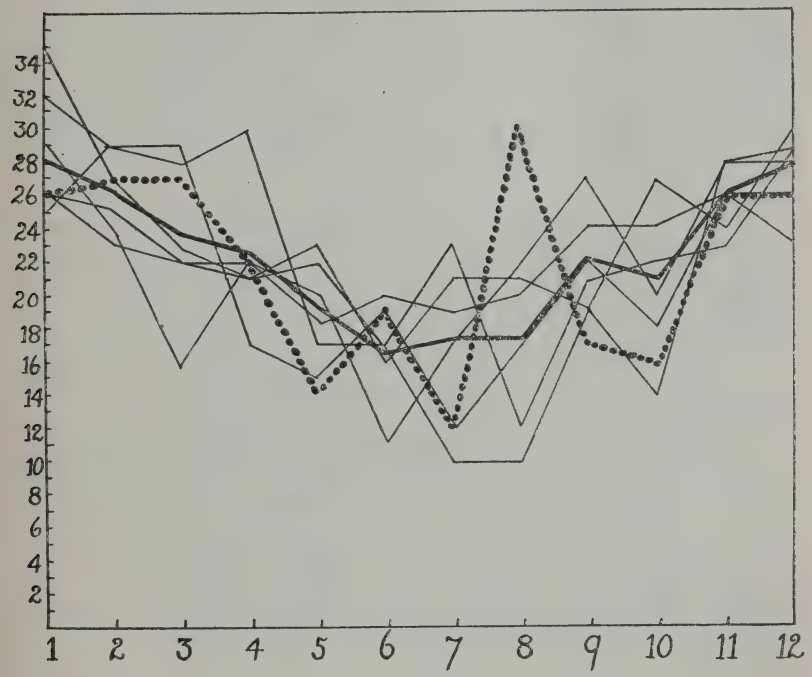

FIGURE 1

[6]In order further to ensure that the experimental conditions of the unexpectedness of the change would be realised at later sittings with new subjects, the subjects were warned, after completion of the Experiment with them, against communicating to others the fact of any such change having taken place. This procedure was also followed in the case of the other Experiments to be described later. From the answers received from later subjects when questioned on this point it appeared that these instructions had been conscientiously followed.

and differences in the difficulty of the different series among each other, have therefore practically neutralized each other in the total results, even should they have been present to a more than negligible extent in particular instances.

The accompanying Figure 1 is obtained from Table 1 and clearly shows the result which is of importance for our purpose.

In Figure 1 the places occupied by the letters in the series are given on the abscissa, the heights on the ordinate corresponding to the scores per place. The thin black lines mark the curve for the total scores for each place in each series preceding the critical one, the heavy black line giving the curve for the means of these scores taken together. The dotted black line forms the curve for the total scores for the letters in the different places in the four critical series. It is easily seen from the figure that in all the series, except the critical one, the number of times in which the letter in the eighth place was correctly recalled was comparatively small. The thick black line shows clearly that the recall, in the case of the non-critical series, has followed the following general law:—the letters at the beginning and the end were recalled best, the recall becoming poorer as we proceed from the ends towards the middle of the series. The result is, of course, in agreement with those obtained in many other memory experiments.

The point of importance for our problem lies in the fact that, in the critical series, we see a very marked rise of the curve for the eighth letter, this being the critical letter. The rise evidently goes well beyond the region of the 'chance' variations occurring in the non-critical series as shown by the thin continuous lines. The rise for the critical letter shows that the disadvantage due to its position has been overcome to such an extent, that its mean score is now, not only far in advance of that for any letters similarly placed in the non-critical series, but that it even exceeds the total mean score (shown by the heavy black line) for the letters in the most advantageous positions, namely that for the letters occurring first in the series. This rise in the curve is obviously the result, in the first instance, of the fact that the letter concerned had a different colour in the critical series from that of all the other letters in a set. We conclude that this must be due to an increased degree of attention having been given the heterogeneously coloured letter.

Experiment II

This Experiment was based on the same general principle, for registering an increased degree of attention, as was used in the case of Experiment I, but the procedure was different with regard to the nature of the homogeneity and heterogeneity which were now introduced. Described in general terms it consisted of unexpectedly introducing an unfamiliar content among a number of others with which the subject was already fairly familiar, and of then comparing

the degrees with which the old and the new could be reproduced by the subjects.

The Experiment was carried out with three sets of nonsense syllables. Each set consisted of six series of nineteen nonsense syllables each. Series one to five in any one set consisted of the same syllables, the syllables being arranged in completely changed orders in the different series of a set. The first five series were the non-critical ones. The critical series, sixth in each set, contained eighteen of the syllables occurring in the non-critical series in a further fresh order, one new syllable being added to the eighteen. The new syllable occurred in the seventh place in one set, and in the tenth and thirteenth places respectively in the two other sets. The syllable selected as the critical one was different in each set, but occurred as an ordinary syllable in either the one or the other of the remaining sets. In these it had the same place in the sixth series as that held by it in the series in which it was the critical syllable. Its position was thus, in all cases, relatively unfavourable for recall. The letters in all the syllables as shown the subjects were of the same colour (a dark grey), and appeared on a white background. By means of a Lipmann memory apparatus (new construction) suitably placed in an epidiascope, the syllables were thrown on a screen, their size, as projected, being about two inches in height, the whole syllable being about six inches in length. By this means it was possible to take the subjects in small groups of from two to five persons. The subjects were placed about four yards away from and in front of the screen.

Sixteen fresh subjects took part in this Experiment, all being again either students at the University or members of the teaching staff. They did not know what the aim of the Experiment was nor the principle on which the syllables were being shown. The greater number had had no experience of experimental work in Psychology. One set only was used with each subject, the sets being distributed among the subjects so that six were shown the first set, five the second set, and five the third set.

First of all the non-critical series one to five were shown in succession to the subjects, who were instructed beforehand simply to watch the syllables attentively as they appeared.[7]

The syllables were shown in succession in each case, the time of exposition being three seconds per syllable. In this way the subjects attained to some familiarity with the nineteen syllables of the non-critical series and thus also with eighteen of the syllables in the critical series before the latter was shown. The continual change of position of the syllables, mentioned above, was introduced in order to prevent the syllables becoming associated with one another as always, or more or less often, coming in a particular order.[8] Should such associations be formed it was to be expected that the subjects would anticipate particular syllables before their appearance when later series were being shown, such as, e. g., the critical series. We intended to examine, at least in the main, the effect of the quality of non-familiarity as heterogeneous characteristic where the other syllables were homogeneous with regard to the quality of familiarity, and to avoid, in the main, a more definite expectation, when the later series were

[7]In a few cases it was ascertained by means of questions set the subjects after the Experiment had been concluded with them, that they had expected that recall would be required and that they had attended to the syllables with a view to this possibility.

[8]Our procedure thus differed in this point from that of Meumann. Cp. note 3.

being exposed, than that for the familiar in general. The subjects did not know that a new syllable would be introduced, and the result was that its appearance was unexpected by them. Immediately after the critical series had been shown, the subjects were asked to write down the syllables which had occurred in it. With a view to ensuring that a special attempt at recall for each particular syllable should be made the following procedure was followed. The first letter of each syllable was called out by the experimenter, the syllables being taken in the same order in which they had occurred in the last, i. e., the critical series, and the subjects having been instructed to write down the syllables beginning with the letter named. Five seconds were given as maximum time for the recall of each syllable. No two syllables in any one series, i. e., also in any one set, began with the same letter; and, similarly, no two in any one series or set ended with the same letter. The series had been prepared in this way in order to avoid, as far as possible, associative or reproductive inhibition of one syllable by another, and of so introducing undesirable inequalities of difficulty, there being the possibility that such inequalities would render comparison of scores difficult, if not impossible or unreliable. The whole Experiment was performed with each subject or small group of subjects at one sitting, the whole lasting about one-quarter of an hour.

The following are the results bearing on our problem. The mean score for all the syllables at recall, except the new one, was 47.5% per place, the places being those the syllables had had in the critical series of each set. There was a mean variation of 8.9% between these scores, the maximum mean score being that of 66.7% of the familiar syllable occurring last in the critical series. The mean score for the new syllable was 58.3%, its score being therefore over 12% more than the mean score of the ordinary syllables. This difference is the more worthy of remark because of the relatively unfavourable positions occupied by the new syllable for purposes of recall, and because of the fact that it lacked the advantage of the five previous repetitions all the other syllables had had, the mean score for some of these being as low as 33.3%. The advantage accruing to the critical syllable because of its heterogeneity in its set is further brought out by the fact that the mean score for the same syllable when falling (again as 7th, 10th, or 13th, according to its place as critical syllable) among the ordinary or familiar syllables, is 44.4%. This is nearly the same as the total mean score of 47.5% for the ordinary syllables. This shows that the fact of syllables, being recalled so well when occurring as critical syllables, cannot be ascribed to their having been remembered better on the ground of other peculiarities than that of having been heterogeneous with regard to their newness.

Experiment III

The aim of this Experiment was to examine the effect due to the heterogeneity of one unit moving among a number of stationary ones, all being presented simultaneously. The units consisted of consonants.

The following arrangement was used by means of which one letter visibly moved among sixteen different ones. The letters were thrown on a screen, by means of a projection lantern, from diapositives having an arrangement of letters as shown in Figure 2.

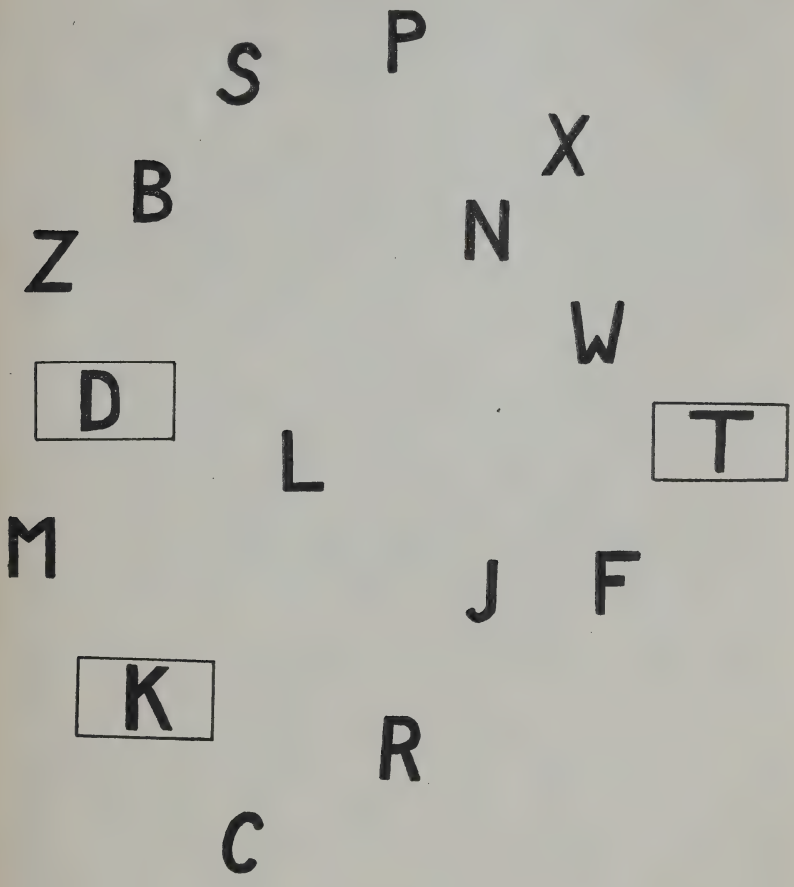

FIGURE 2

The letters fell on the screen within an area of a diameter of about 30 cm. The diapositives, which were used in pairs, were made by pasting pieces of transparent colourless celluloid paper over circular openings in pieces of stiff cardboard. The whole transparent surface of one of each pair of diapositives was then covered with an opaque white paint, leaving only those parts representing the fifteen stationary letters uncovered, and in addition, one small rectangular space on each surface. In the case of the second diapositive of a pair the opening in the cardboard was made

somewhat larger, and the transparent paper pasted over the opening was left uncovered with the exception of a somewhat larger rectangle than that left uncovered in the case of the first diapositive of a pair. On the covered rectangle of the second diapositive only the part representing a letter was left uncovered. The covered rectangle in the second diapositive corresponded in position with the uncovered rectangle of the first diapositive. When, thus, the two diapositives of a pair were properly placed over each other in the lantern, the sixteen letters were thrown on the screen. By now moving the second diapositive slightly forwards and backwards by hand over the surface of the first, the letter on the covered rectangle was caused to move similarly, though to a greater extent, on the screen, the letters from the first diapositive remaining stationary. The fact of the covered rectangle being larger, to a sufficient extent, than the uncovered rectangle on the first diapositive, prevented other light than that of the letters being projected on the screen at the time of movement. Similarly, the fact of the opening in the second diapositive being larger than that of the first, prevented the disappearance, during exposition, of any letter from the screen as a result of the edges of the cardboard of the second diapositive interrupting the course of the projected light. The letters all appeared as white on a grey background, the expositions taking place in weak diffused daylight. Three different pairs of diapositives were used, but so made that the same sixteen letters, irregularly distributed, as shown in Figure 2, appeared on the screen in each case. The only difference that existed between the groups shown by the three pairs of diapositives consisted of the fact that the moving letter with one pair of diapositives was a stationary letter with another pair of diapositives. By means of experiments previous to the main one, and carried out with other subjects than those used in the main Experiment, it was found that letters in certain positions, indicated by the rectangles in Figure 2, fell among those that were comparatively badly remembered. The letters in these positions (K, D, and T) were selected for the purpose of being moved. The size of the rectangles indicates with close approximation the relative extent of the movement.

At the beginning of the Experiment the subjects, who were usually taken in small groups and placed about four yards from the screen, were first of all instructed to attend to what would appear on the screen. They did not know that a moving letter would appear among stationary ones. An exposition, lasting fifteen seconds, without interruption, then took place, the one letter moving backwards and forwards all the time at a rate which still allowed of its being easily recognizable. Immediately after the exposition the subjects were required to write down all the letters remembered with at least reasonable certainty. Only one pair of diapositives was used and one exposition given with each of the fifteen subjects taking part in this Experiment, the numbers used with each pair of diapositives being nearly the same in all cases. All the subjects were University students, but most of them were quite untrained in any experimental psychological work. They were all ignorant of the aim of the Experiment and did not know beforehand what was to be shown on the screen.

The following were the main results: The maximum mean score for all the subjects for a stationary letter was 80%, that being the score for Z; the lowest mean score for a stationary letter was 0% for F. The mean score for all the stationary letters was 42%, with a mean variation of 19%. Table 2 gives the data relating to the

letters K, T, and D when moving and when stationary. In the table, *a*, *b*, and *c* indicate the groups of subjects used with each of the three pairs of diapositives.

TABLE 2
EXPERIMENT III

Subjects	Letter	Stationary	Moving
a and *b*	K	40%	100%
b and *c*	T	20%	100%
a and *c*	D	10%	100%

The table shows that the mean score for the letter K when stationary (and thus homogeneous in this respect with the mass of the others) obtained by groups *a* and *b* was 40%, whereas the score for K when moving obtained by group *c* was 100%, and so on for the other letters.[9] We conclude that here again the heterogeneous units obtained a greater degree of attention from the subjects.[10]

EXPERIMENT IV

The aim of this Experiment was the examination of the effect on attention of the heterogeneity of one unit being stationary among a number of moving ones. In this case also the units consisted of consonants, and all were shown simultaneously.

Twelve consonants, grouped irregularly as shown in Figure 3, were projected on the screen. The dimensions of the objects shown were the same as in the immediately preceding Experiment. The same held true of the general conditions and time of exposition, as well as of the distance of the subjects from the screen. Here, too, pairs of diapositives were used. They were prepared in the same general way as those used in Experiment III. In the present case, however, the moving diapositive contained eleven letters, and the stationary diapositive one letter. Three pairs of such diapositives were used. The letter remaining stationary with one pair always

[9]This method of comparison follows, of course, from the conditions of the Experiment. It was thus, e. g., not possible to compare the scores for, say, group *c* with respect to K when stationary and when moving. Our method of comparison, therefore, implies the assumption (which we consider ourselves justified in making) that also for group *c* the letter K would have received a score considerably under 100% if it had been a stationary letter for that group, since only 40% was obtained as score for this letter by groups *a* and *b*. Similarly for the letters D and T.

[10]The view has been advanced that movement is one of the determinants of attention since "Moving stimuli affect different nervous elements in quick succession; there is no possibility of fatigue or nervous adaptation; in a way, therefore, the action of the moving stimulus is cumulative." E. B. Titchener, *Textbook of Psychology*, 1916, 270-1. Cp., also his *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 205, where it is stated in a note (p. 359) that he is there following G. E. Müller's *Sinnliche Aufmerksamkeit*. The relative unimportance of such cumulative effect on the nervous system resulting from the mere movement of the critical letters in Experiment III is shown in our discussion of the results of Experiment IV.

fell among the moving letters for one other pair, but had the same position (apart from the small movements made by it in the one case) when stationary, as when homogeneous with the mass of letters being shown. The letters placed in rectangles in the figure are those which remained stationary at one or other of the expositions. The size of the rectangles in the figure again indicates to a close approximation the extent of the movement.

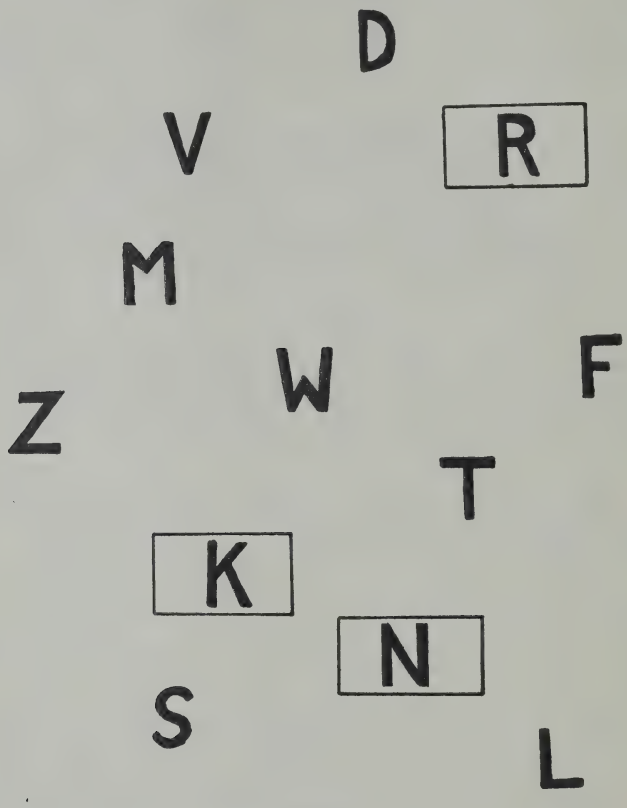

FIGURE 3

The Experiment was again carried out with one exposition only with each subject, the pairs of diapositives being distributed equally among the 24 subjects taking part in the Experiment. The subjects were of the same kind as those used in the previous Experiments, but were different persons. They were ignorant of the aim of the Experiment, and they did not know beforehand that a stationary unit would be shown among moving ones. Immediately after the exposition they were required to write down all letters remembered by them with at least reasonable certainty.

The following are the main results having a bearing on our problem:—The mean score per moving letter was 59.9%, with a mean

variation of 13.4%. The maximum mean score for a moving letter was 79%, that being the score for M; the lowest score was that for K when moving, namely 37.5%. Table 3 gives the necessary data relating to the letters R, N, and K when moving and when stationary respectively, these being the only letters occurring in both ways. The groups of subjects are again indicated by *a*, *b*, and *c*.

TABLE 3
EXPERIMENT IV

Subjects	Letter	Moving	Stationary
a and *b*	R	75%	87.5%
b and *c*	N	64.5%	100%
a and *c*	K	37.5%	100%

Calculating from the data given in Table 3 we see that the mean score for R, N, and K when homogeneous with regard to the mass of the letters was 59.1%, and when heterogeneous it was 95.8%. Applying the same method of comparison as that used in the case of Experiment III, we thus again find a very marked difference in the scores for the letters when homogeneous and when heterogeneous, and again conclude that this difference must be due to an increased degree of attention having been given them when heterogeneous.

At this stage attention may be drawn to a preliminary point. In estimating the importance of a moving stimulus for attracting attention, we have, of course, to distinguish between stimulation of fresh parts of the nervous system resulting from a) the movement of the subject's eyes during perception, and b) the movement of the object itself which is perceived. In the case of Experiment III both were present, since there can be no reasonable doubt that the subjects did move their eyes in attentively observing the letters thrown on the screen at exposition, and since the attempt to fixate any fresh letters would lead to the stimulation of further portions of the retina by the moving letter. The perception of each of the letters would be affected by the first mentioned type of cumulative stimulation, while only the perception of the moving letter would be affected by both types of such stimulation. According to a current view (cp. note 10) as we understand it, emphasis is to be laid on the second type of cumulative stimulation in explaining the effect on attention of a moving object. The examination of the conditions obtaining in Experiment III already suggests the possibility that such stimulation may be relatively unimportant for causing the results obtained there. For, even if we assume that the subjects made comparatively few eye movements in fixating different letters in turn, the extent of the shifting of the images of the stationary letters over the retina would have been considerably larger than the relatively small excursions made by the image of the moving letter and due to this movement itself. If, then, the cumulative effect of the stimulations of fresh portions of the nervous system were to be the sole, or even the main, cause for the moving letter attracting attention in Experiment III, we should incline to the belief that the differences between the scores for K, D, and T when stationary and when moving, should not have been so large as they actually were. It is now clear that this suggestion is strongly confirmed by the results of Experiment IV; for,

in this case, the letter to which only the first type of cumulative stimulation accrued attracted a higher degree of attention than the same letter obtained when receiving both types of cumulative stimulation (accepting here also, as we may, that eye movements on the part of the subjects took place). We thus conclude that it is not the *mere movement only*, in the field of perception, of a moving object that causes attention to be drawn to it, but that attention is drawn to it, at least also, because and when its movement gives it the characteristic of heterogeneity with other objects. It is clear that this result accords with Selz's *Gesetz der Distinktionsbeachtung.*

The Experiments so far described all agree in showing a markedly increased memory effect for a heterogeneous content unexpectedly introduced among a group of otherwise homogeneous contents. We have interpreted this result as being due to a superior degree of attention having been given the heterogeneous content. In the Experiment to be described next this is shown in another manner.

Experiment V

This Experiment was carried out with the aim of studying the effect on attention (and reaction) of the sudden and unexpected cessation of an auditory stimulus and sensation. It forms a continuation of the considerable amount of work that has already been done in which reaction has been used as a means of studying various aspects of attention. Since the cessation of the sensation was used in this Experiment as a means of distraction, the Experiment also belongs to the line of work in which the influence of distractors on attention has formed the subject of examination.

In our case the general principle was to have a comparatively loud noise going on during the course of the experimental period, during which the subject was performing simple reactions, the sound ceasing suddenly and unexpectedly (as far as the subject was concerned) just before the visual stimulus for reaction came in one (the last) case, this being the critical reaction.

The details of the arrangement of the apparatus, as used in the Experiment, are shown schematically in Figure 4. The subject (S) is seated in a quiet room (1) with a sound hammer near him. In front of him lies, on the table, the reaction key (K), and about two feet in front of and away from him is a large screen (Sc) which prevents the subject from seeing the apparatus or the experimenter (E 1) on the other side of the screen. The screen had two small openings (O 1 and O 2) at eye level, the two being about six inches apart and ½ an inch square. Both were clearly visible to S in the light of the window to the back of him. Behind the screen, and just above the openings O 1 and O 2, two electromagnets (M 1 and M 2) had been placed in fixed perpendicular positions. P 1 and P 2 are pieces of stiff white cardboard which filled the openings in the screen when held up against the lower surface of the electromagnets and with their surfaces parallel to the perpendicular surface of the screen. To both P 1 and P 2 are firmly attached soft iron rods with broad heads. To

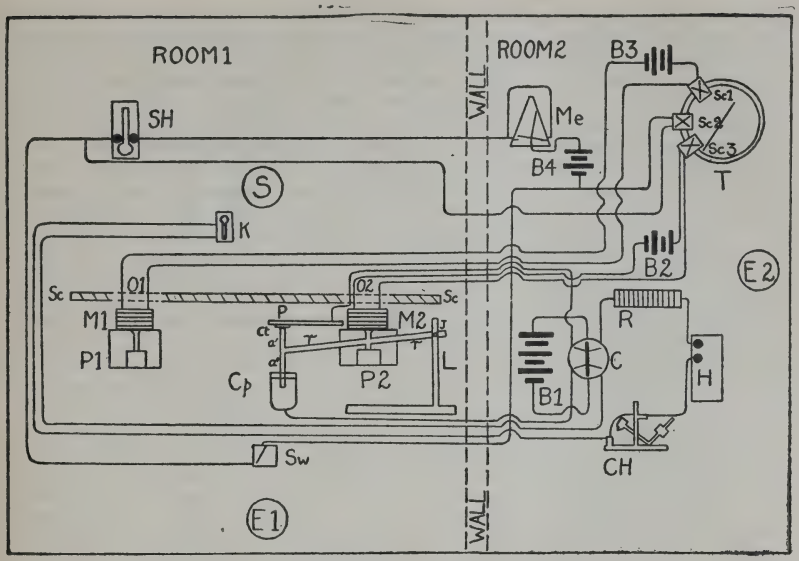

FIGURE 4

both P 1 and P 2 were furthermore firmly attached small heavy weights, P 2 being additionally weighted in a manner to be described later. On suitable electrical currents being passed through M 1 and M 2 the pieces of cardboard were thus held in position by means of the heads of the iron rods pressing against the cores of the electromagnets, the cards dropping down as soon as the currents passing through M 1 and M 2 were cut off. Both P 1 and P 2 were attached by means of bits of string to a support (not shown in the figure), both the strings and the support being on the experimenter's side of the screen. The length of the string attached to P 1 was so chosen that it prevented the weighted card reaching the table in falling, so that the drop was noiseless. The string was, however, long enough to allow of P 1 disappearing completely from the subject's view when it had fallen. P 2 was firmly fixed to an iron rod (r), the latter moving easily at the join (J) with the lever support (L). On the side opposite to L the rod had two perpendicular arms (a′ and a″). On P 2 pressing against M 2 the arm a′ presses against the platinum contact point (Ct) on the lower side of the steel plate (P), the latter also having a fixed position approximately on a level with the lower surface of M 2. A portion of the arm a″ dips into the mercury of the mercury contact cup (Cp), the latter also having a fixed height above the table, and so chosen that a″ still dipped into the mercury when a′ was pressing against Ct. Cp was deep enough to allow of a drop of P 2 clearly visible to the subject, without a″ striking the bottom of the cup; this being prevented, in addition, by means of the string connecting P 2 with the support. The drop of P 2 was thus also made noiseless. The whole of the arrangement connected with M 2 and P 2 was thus an adaptation of the *Federpendal* Tachistoscope after Wirth as supplied by Zimmermann. Sw is a switch by which E 1 could cut off an electric current passing through

the electromagnets of the sound hammer. In room 2 there is a mercury contact metronome (Me), used for actuating the sound hammer in room 1, and set to give 100 contacts per minute. A rapid and continuous tapping of the sound hammer was thus caused. The tapping varied slightly in intensity at times. T is the large time sense apparatus. The hand used with it was moved by means of the large 'Universal' kymograph (not shown in the figure) as supplied for this purpose by Zimmermann. It found (by stopwatch timing) that this kymograph, if properly wound each time, moved the hand at a speed of considerable regularity and for a far more sufficient time for our purposes. On T were three 'star' contacts (Sc 1, Sc 2, and Sc 3). These were set, and the movement of the hand so regulated (by stopwatch timing), that 1½ seconds elapsed between the breaking of the electrical contacts at Sc 1 and Sc 3 respectively, and ½ of a second between the breaking of the contacts at Sc 2 and Sc 3. Sc 1 and Sc 2 were so connected with M 1 and M 2 in room 1, that the currents from the batteries B 2 and B 3 had to pass through the 'star' contacts in order to reach M 1 and M 2 respectively. Sc 2, B 4, Me, Sw, and SH were so connected that (1) SH would keep on tapping even if the current were broken at Sc 2, as long as the current were not also broken at Sw, and (2) breaking the current at Sw, but not at Sc 2, would cause the sound hammer to go on tapping. The timing took place with a Hipp Chronoscope (H) which was regularly tested during the experiments with the control hammer at 243 sigma and showed a mean variation of just under one sigma. No appreciable error in the running of the clockwork itself could be ascertained by the (only available) method of stopwatch control. The lower magnets were connected up on a make-to-break shunt circuit, the chronoscope, the resistance (R), the control hammer (CH) and the subject's reaction key (K) in room 1 falling in the one circuit leading to the commutator (C). On the shunt circuit we have the mercury in Cp, the metal arms a′ and a″ and the plate Pt with the contact Ct. A second experimenter (E 2) was situated in room 2, which was separated from room 1 by a thick wall (there being no doors in the wall). The wires passing between the two rooms went through a small hole in the wall, the hole being further blocked up with wadding, so that the sounds coming from room 1 were not heard by the subjects during the experimental period.

The Course of the Experiment. The subject was instructed at the beginning to react as quickly as possible on the dropping of P 2 by pressing down K on which the forefinger had to be kept in readiness. He was also informed that the dropping of P 1 would act as warning signal. The subject had, thus, first of all to watch for the warning signal, and on its occurrence he had to turn his eyes to O 2 in preparation for the stimulus for reacting. Since the metronome had previously been set going by E 2, E 1 now set the sound hammer tapping by switching on the current at Sw. E 1 now also signalled to E 2 by pressing the button for an electric bell in room 2. (The latter arrangement is not shown in the figure.) On this signal E 2 depressed the commutator and set the chronoscope going. He then made contact at Sc 1 and Sc 3 by adjusting the 'star' contacts by hand, thus sending electrical currents through M 1 and M 2. E 1 had in the meantime been holding P 1 and (or) P 2 in suitable positions against M 1 and (or) M 2, so that the cards remained hanging on contact being made at Sc 1 and Sc 2. The fact that the cards remained hanging acted as signal to E 1 that the moment had arrived for giving the subject a preliminary warning to get ready and to signal 'ready' to E 2 by means of the bell. On this signal the latter set the hand of the time sense apparatus moving, letting it start with its contact-breaking point in a position just next to Sc 3 as shown in the figure, so

that the hand should have attained an even pace by the time it reached the three 'star' contacts. On the current being broken at Sc 1, the card P 1 dropped, and 1½ seconds later P 2 dropped on the current being broken at Sc 3. As P 2 started dropping the shunt current connected with the commutator was broken at Ct, so that the current from the battery B 1 now passed through the lower magnets of the chronoscope; the hands of the latter now started moving, to stop when the subject reacted by pressing on his reaction key, thereby breaking the chronoscope's own circuit. E 1 now waited a little time after having observed the dropping of the cards, and then signalled 'ready for the next' to E 2 by ringing the bell. The procedure just described was then repeated, and so on.

After the subject had been given a number of preliminary trials, the main Experiment began with him. This consisted of ten 'ordinary' reactions with each subject,[11] the hammer tapping without interruption during the whole of this part of the experimental period with a subject[12] (as well as for the later part of the period in which the trials were being given). The eleventh (critical) reaction now followed. For it the following changes were made in the procedure, but without the subject's knowledge:— (1) On E 2 receiving the 'ready for the next' signal from E 1, he now set Sc 2 so as to make the contact for the current going through Sc 2 to the sound hammer, and (2) E 1, after waiting a moment to allow of this being done, switched off (at Sw) the current passing through the sound hammer along the wire in which Sw lay; the fact of the cards remaining hanging acted as signal to E 1 that this could now safely be done. The result of these changes was that no current could now pass through the metronome and the sound hammer, after the hand of the time sense apparatus had broken contact at Sc 2. The consequence was that after P 1 had dropped, and shortly before P 2 dropped, the hammer suddenly stopped its loud tapping and remained quiet.[13] After the critical reaction the subject was required to give an introspective account of it. The whole experiment took about ¾ of an hour with each subject.

The ten subjects taking part in the Experiment were all students of Psychology at the University and all possessed a fair degree of experience in introspecting. None of them knew what the aim of the Experiment was, nor of the reason for the tapping of the hammer. They did not expect that there would be any cessation of the noise, as was ascertained from them after giving their introspection if they did not mention it of their own account in their report.

We turn, firstly, to the study of the reaction times. Table 4 gives the eleven (in one case ten) reaction times in sigma for each of the subjects, the times being given in the same order in which the reactions took place; the critical reaction time (C.R.T.) thus comes last of all. In the column immediately preceding it is given the mean of the ordinary reaction times (M. O. R. T.) to the nearest sigma for each of the subjects. The last line of the table gives the mean of each of the columns of reaction times.

[11] By mistake only nine of these was taken with one of our subjects.

[12] See, however, note 17.

[13] The exact moment at which this took place varied for the different subjects since there was only one tap every 3/5 of a second.

TABLE 4

EXPERIMENT V

Subj.	1	2	3	4	5	6	7	8	9	10	M.O.R.T.	C.R.T.
VdM.	207	274	220	240	443	192	191	180	174	181	231	349
Lo.	199	234	228	223	232	254	256	272	246	225	242	260
laG.	184	190	194	325	201	275	213	256	184	184	221	230
Mar.	125	192	136	143	156	147	138	153	157	178	152.5	153
Bur.	174	153	325	174	178	173	172	196	266	167	198	373
He.	190	188	171	149	213	188	189	201	188	187	186	275[14]
vSt.	187	149	184	174	169	181	165	229	161	166	176.5	195
Grb.	184	218	285	236	402	390	248	259	272	315	281	341
Vis.	171	443	180	207	191	195	203	180	187	201	216	196
Bot.	—	177	177	170	166	174	217	185	178	397	205	206
Means	203	222	210	204	240	217	209	211	201	220	214	258

The Table shows that in 9 out of the 10 cases the subjects' reaction times were longer than the mean of the ordinary reaction times for the individuals (M. O. R. T.); it is true, however, that in two of the cases (Bot. and Mar.) the difference is so small as to be negligible. In the case of the subject Vis. the mean ordinary reaction time for him is actually longer than the critical reaction time.[15]

The mean critical reaction time for all the subjects together is 20.5%, of the total mean ordinary reaction time, longer than the latter, none of the means of the columns of the ordinary reaction times showing so large a deviation from their own mean. The difference (of 44 sigma) between the total mean critical reaction time and the total mean ordinary reaction time is almost four times the size of the standard deviation (11.1 sigma) of the means of the first ten columns of ordinary reaction times.

The protocols, also, leave no doubt as to the cause systematically acting in the direction of bringing about this average increase of the critical reaction times; it is, namely, that the subject's attention was attracted to the sudden cessation of the noise. Two constituents of this distraction from the reaction should, however, be distinguished:—(1) the primary distraction, i. e., the mere fact of the subject noticing that the sound had stopped, and (2) the secondary distraction, i. e., further processes arising in consciousness as the result of the primary distracting content. In the main (2) took the form of the subject attempting an explanation for the cessation. As a result of the situation in which the subject was at the time, this attempted explanation—quite naturally—took a stereotyped form. The following are the

[14]See note 17 to this subject's introspection.

[15]It may be pointed out that in the case of Bot. and Vis. there were two exceptionally long ordinary reaction times (Bot. in the 10th column has 397 and Vis. in the 2nd column has 443 sigma). In these cases some strong chance disturbance must have been present. Apart from these two reactions the critical reaction times are, for both subjects, longer than the mean reaction times for the ordinary cases, M. O. R. T. being then for Bot. 180 and for Vis. 191 sigma. We return, in the introspective protocols, to the case of Mar.

protocols. For convenience of reference the mean ordinary and the critical reaction times of each subject are again given, between brackets, after his designation. The mean ordinary reaction time for the subject comes first.

1. VdM. (231-349) "When the first card fell I concentrated on the perception of the second card as usual. Just before I was going to react the hammer stopped sounding and this drew my attention. I looked for the reason for this variation and wondered if anything had gone wrong. Just at this moment the second card fell and some confusion arose in my consciousness. I reacted more or less automatically and fairly quickly, but I was aware that the reaction had taken longer than was usually the case. Then followed a mingled feeling of relief but also of disappointment at the reaction not having succeeded so well as usual."

2. Lo. (242-260) "I was listening to the noise when the first card fell, and I heard the noise echoing after it had stopped. I was waiting for the noise to go on when the second card was about to fall. I thought something had gone wrong with the apparatus, but this was only after reaction. The stoppage of the noise seemed to come at about the same time as my reacting."

3. laG. (221-230) "When the first card fell my eyes went to the second card. I was then keenly prepared to press the reaction key as soon as this card fell. I had decided to react as quickly as possible, and after the fall of the first card there was a feeling of tension in my right hand. On perceiving the fall of the second card I immediately reacted. As this card fell I noticed that the sound hammer had stopped and at once thought that something had gone wrong with the apparatus. I never expected the sound to stop."

4. Mar. (152.5-153) "I noticed the stoppage of the sound, having been conscious of the sound itself all the time. I had noticed irregularities in the noise and wondered if they affected the experiment, and there was an inclination to look round to see what was causing the sound. When the noise stopped I thought the experiment was finished, but I have the impression that this thought came only after reaction. I never expected the sound to stop. Just before the last reaction I had become a little careless and I had thought that I must now concentrate attention on reacting."[16]

5. Bur. (198-373) "I gave a lot of attention to the noise at my back. Earlier in the experiment I had noticed that it seemed to change at times, and I had wondered what that had to do with the experiment. I attended strongly to the two cards and to reacting quickly when the second fell. I noticed that the sound stopped and this surprised me. I had not thought that the sound would stop, and I thought that the experiment would be coming to a stop now. The stoppage of the sound did not hinder me in reacting." (Note:—The subject's long critical reaction time shows that this last impression was incorrect.)

6. He. (186-275) "I was intensely ready to move my eye to the second opening after the card in the first had fallen, and was strongly aware during the whole time of the noise at my back. The greater my tension was the more the noise at my back seemed able to distract me. I did sort of notice that the sound stopped, but attached no importance to it at the moment. At

[16]This agrees with the comparatively long reaction time, for this subject, of 178 sigma in column 10, and possibly explains why his critical reaction time shows only a negligible lengthening, since a special decision to attend to reaction had evidently been taken for this last case.

the earlier occasion when the mistake was made,[17] I had been startled by the previous stoppage and had reacted prematurely. I notice, however, that the cessation of the sound in the second case caused me to lift my finger from the reaction key without waiting in the same way as ordinarily."

7. vSt. (176.5-195) "I fixed my attention on the first card and, after its fall, on the second, strongly expecting that the latter would now also soon fall. My attention was given to this card principally, and as it fell I reacted without thinking of reaction. I was just attending to the stoppage of the noise when I saw the card fall and I reacted immediately. I gave no further thought at the time to the stoppage of the noise. It only attracted my attention in so far as my attention went from the card to the stoppage. I have the impression that I thought after reaction that there was possibly a mistake in the experiment."

8. Grb. (281-341) "I voluntarily attended to the first card and saw the second only indirectly . . I was conscious of sensations of touch of my finger on the key. I did not again repeat the decision to react as soon as the card fell, but I was in a state of tension and pressed my teeth together. Just before I reacted I noticed that the sound had stopped, and I thought that the current must have been cut off, but I did not further attend to the stoppage and it did not put me off. (Note:—The subject's reaction time shows that he is mistaken on this point.) I never expected the sound to stop. I thought the continuous noise was meant to tire me."

9. Vis. (216-196) "I was clearly conscious of the cessation of the sound; the decision to react had been taken and when the card fell I reacted automatically. After reaction I wondered whether the stoppage meant the end of the experiment. I do not remember further thought about the stoppage before reacting, but only noticed that the sound had stopped. I never expected that it would do so. I thought that the sound had something to do with the working of the exposures."

10. Bot. (205-206) "The sound stopped when the first card fell, and I was in doubt whether I should proceed as usual and whether the experiment was being carried out properly. I decided to do as I had been instructed. I was slightly confused and wondered whether I should not look upon the stoppage as part of the experiment. I did not expect it to stop." (Note:—It appears that the period between the stoppage of the sound and the stimulus for reaction was long enough in this case to allow of attentional recovery.)

The result of this Experiment[18] thus agrees fully with the results obtained in Experiments I to IV with regard to the influence of an unexpected heterogeneity in drawing attention.

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[17]In the case of an earlier reaction during the experimental period the tapping of the hammer had been stopped by mistake just before the signal for reacting had occurred, and the subject had then reacted before the second card fell. The subject had been informed that this stoppage was a mistake so as to prevent his expecting the later (critical) stoppage of the sound. His reaction time in the critical case, as well as the protocol, still shows, however, the disturbing effect of this second stoppage. The rest of the protocol refers to the instructions given the subjects to keep the key down for a short time after reacting.

[18]Cp. W. Wundt, *Physiol. Psychol.*, 3, 1911, 416:—"Mehr noch wird die Reaktionszeit bei *vollig unerwarteten* Eindrücken verzögert. Diese Bedingung ist bei den Reaktionsversuchen durch Zufall bisweilen verwirklicht, wenn der Beobachter zerstreut ist. Absichtlich lässt sich aber das nämliche herbeiführen, wenn man in einer längeren Versuchsreihe mit regelmässigen Inter-

Experiment VI

Our second group of Experiments, of which VI forms one, was carried out with the aim of examining the effect on attention and reaction of an unexpected change in the quality of the reaction stimulus; the change being, however, of such a nature, that the stimulus would still be able to elicit a prescribed reaction.

The general principle on which the Experiment was based consisted of the subject's performing a number of simple reactions on one of a series of four letters shown on cards, the four letters being exposed simultaneously. All the letters on all the cards used with a subject were homogeneously coloured. To this rule there was one exception. On the critical card, which was always shown last, the letter serving as stimulus for reaction had another colour than on the preceding cards, without the subject expecting that such a change would be made.

Figure 5 shows schematically the nature and arrangement of the apparatus used. The subject (S) is seated directly in front of the Menzerath exposure apparatus (M) as supplied by Spindler & Hoyer, the light falling on the aperture of M. The aperture was just below eye level and about two feet from the subject's eyes. K is the subject's reaction key, and CH is the control hammer. The screen (Sc) separates the subject from the experimenter (E) and the rest of the apparatus. Timing took place with the Hipp Chronoscope (H) which was regularly tested throughout the experiments with the control hammer at 280 sigma and showed a mean variation of just over one sigma. No reliable method of testing for longer times was available as far as the working of the springs and electromagnets was concerned, so that we had to rely here on the findings of Ach.[19] The lower magnets of the chronoscope are connected on a direct current make-and-break circuit with the regulating resistance (R), the commutator (C), the control hammer, the exposure apparatus, and the subject's reaction key. The contacts of the exposure apparatus were used for completing this circuit when the shutter of the apparatus fell, the circuit thus being completed at the moment of full exposition of the card with the letters visible in the aperture. The electromagnet of the exposure apparatus was actuated by

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vallen der Reize plötzlich ohne wissen der Versuchsperson ein viel kürzeres Interval nimmt."

In distinction from our Experiment V the stimulus cannot be said, in the type of case Wundt has in mind, to have been fully unexpected itself, but merely with regard to the moment of time at which it would occur. Furthermore, in our case the stimulus for reaction did not occur prematurely from the objective point of view. There is, however, the essential point of agreement between the two types of cases, that in our Experiment, too, the stimulus for reaction would tend to occur prematurely in the sense that the subject had not fully recovered from the distraction before the stimulus for reaction came. Our own results are in agreement with the findings of those who state that the degree of attention on the reaction varies inversely with the length of the reaction time. Cp. K. M. Dallenbach, The Measurement of Attention, *Amer. J. Psychol.*, **24**, 1913, 483-484, and H. Henning, *Die Aufmerksamkeit,* 1925, 62-64.

[19] N. Ach, Über die Willenstätigkeit u. d. Denken, 1905, 265-266.

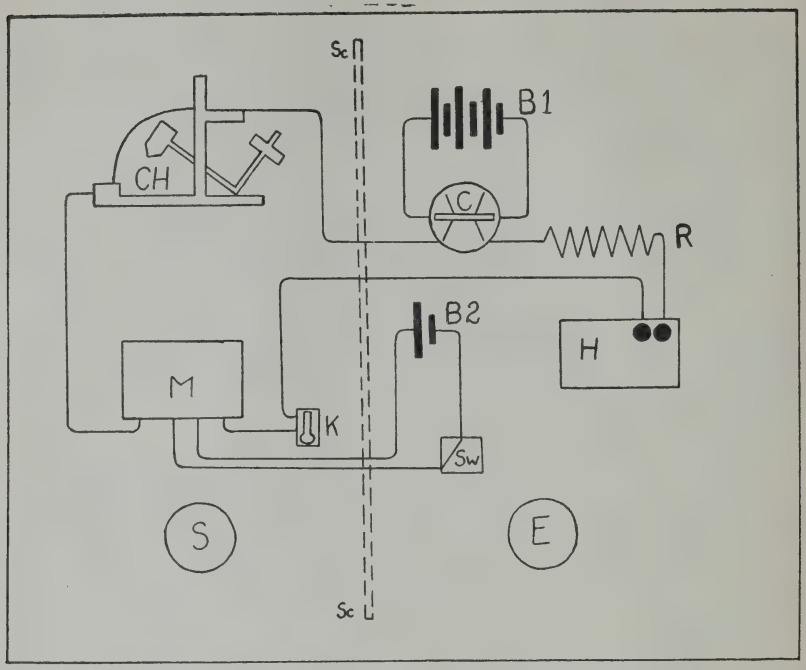

FIGURE 5

the battery (B 2) as soon as the experimenter switched on the current at the switch Sw, thus causing the shutter to fall and expose the card. Six sets of white cards were made use of. Each card had on it four capital letters (consonants) clearly made in bold colours. The letters were printed carefully by hand so as to be of the same type and height (of one centimeter), and to be the same distance apart from each other. The four letters on a card covered about five centimeters length all together. They always appeared in a line in the same place in the aperture of the exposure apparatus. The letters themselves varied from card to card, but those on all the cards of a set had the same colour, with the exception of one letter on the critical card of a set, as already mentioned, which was always exposed for reaction last of all. One particular letter (the reaction letter) occurred on each card of a set, this letter being, in a set, the same as the heterogeneously coloured reaction letter on the critical card. The reaction letter on this card had, thus, not only a different colour from all letters on the ordinary cards of a set, but also from the other three letters on the critical card. In the case of three sets all the letters were blue, with the exception of the critical reaction letter, which was red. The fourth set consisted of red letters with the critical letter in blue. In the fifth set all the letters were green, the critical reaction letter being black. In the sixth set all the letters were black with the critical one in green. Four different letters were used in the sets as reaction letters, namely B, K, L, and M. B was used for this purpose on three of the sets (the first, second, and fourth), the three remaining let-

ters occurring as reaction letters in the other sets. The position of the reaction letter varied from card to card of a set, taking the positions, 1, 2, 3 and 4 an equal number of times in each set, the sets having eight cards each. On the critical card the reaction letter occurred in place 1 in one set, in place 2 in two sets, and place 3 in two sets, and in place 4 in one set. It was found that certain letters showed sufficient similarity with the reaction letter momentarily to attract attention and so to delay attention (such as P or D in the case where B was the reaction letter of a set), this happening notwithstanding the fact that all letters were clearly and simply made. Care was taken, therefore, to omit such disturbing letters from all cards for which the reaction times were to be compared with that on the critical card, such omission taking place also for the critical card itself. The order in which the ordinary cards followed each other was an irregular one with regard to the positions of the reaction letters; this was done in order to prevent the subject developing an expectation for the reaction letter as coming in a definite position.

Some of the thirty-four[20] subjects who took part in this Experiment had had no previous experience at all in experimental psychological work, and others had had very little. Subjects indicated by (t) below are those whose experience of experimental and introspective work was either fair or considerable. With the exception of one (Str.) who was a member of the staff of the Department of Psychology, all were students at the University. The six sets were distributed among the subjects as shown later in Table 5, one set being used with each subject. All the subjects were unaware of the aim of the Experiment, and expected no change in the colour of the letters. A few of the subjects had taken part some months previously in the similar Experiment VII described below. In such cases it was ascertained immediately after the conclusion with each of them of the Experiment now being described, whether they had had any expectation of a change of colour. No further use is here made of any of the cases in which such expectation was present; in the other cases subjects common to both Experiments are indicated by having the same designation in the statements of the results.

Course of the Experiment. The subject was informed at the beginning that the aim of the Experiment was the measurement of his reaction time. He was shown a practice card in the aperture of the exposure apparatus, the drop of the shutter (by means of which the exposition took place) being demonstrated; and he was instructed to react as quickly as possible, by pressing down the reaction key with the forefinger, as soon as he recognized with certainty the special reaction letter of the set, this letter being named by the experimenter. He was also informed of the fact that the warning signal 'ready' would be given vocally by the experimenter shortly before the shutter dropped, and that the forefinger was then to be held in readiness on the key, while the centre of the shutter was to be fixated. He was furthermore emphatically warned not to react on a card not showing the reaction letter specified.[21]

[20]See, however, note 22.

[21]Cards not showing the stimulus letter were introduced among those used for practice purposes with the aim of training the subject to wait for clear recognition of the reaction letter before reacting. Such cards did not occur in the sets used for the main Experiment itself. It was to be expected that disturbing effects would result if such training of the subjects, to wait for clear recognition of the reaction letter, were delayed until, or continued during, the main Experiment. Such disturbing results would consist, e. g., of sudden accesses of exceptional caution, leading to unnecessarily long reac-

Notwithstanding the warning, reactions on 'catch' cards often took place with our untrained subjects in the practice series, and this series was accordingly extended with each subject until such incorrect reactions, if any, ceased. In addition the subjects were instructed to inform the experimenter should they ever react, before clear recognition of the reaction letter, in such cases where it was actually present on the card exposed. No use is here made of the few reaction times in which this was so reported. After the instructions had been given and the shutter closed, the experimenter moved a stimulus card to the required position in the exposure apparatus, and started the chronoscope. The warning signal was then given, and after the lapse of a few seconds (this time the experimenter made as constant as he was able), contact at Sw was made by him, the shutter then dropping at M. In this way the current passing through the chronoscope was completed at the moment of full exposition, and the hands of the chronoscope began moving. They stopped when the subject pressed down his reaction key. The subjects had been requested and trained during the practice series to keep down the key for a short time after reacting, as the hands of the clock would otherwise start moving again and thus possibly prevent a reading of the time being taken.[22] The experimenter broke the current at the commutator as soon as he observed that the hands of the chronoscope had stopped moving. After the last reaction (i. e., the one on the critical card) of the set used with a subject, the experienced (t) subjects were asked:—"Please give me your introspection on the last case." The inexperienced subjects were asked:—"What happened that time?" If these questions elicited nothing or little that was relevant, the subjects were asked whether any change had been observed, and whether they had had the impression that their reactions had taken place more, or less, easily than usual. The time used for carrying out the Experiment with one subject was about twenty minutes and usually less, the whole being finished in one experimental period with him.

We have found it convenient to present and discuss the results of Experiments VI to VIII together, and now proceed to the statement of the procedures followed in Experiments VII and VIII.

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tion times, just after observation of, or mistaken reaction on, an immediately preceding 'catch' card with the proper reaction letter omitted. Cattell made reaction experiments on visual impressions "bei denen, wie gewöhnlich 'Jetzt' gesagt und der Chronoskop in Gang versetzt, aber der Lichtreiz nur in der Hälfte der Fälle hervogerbracht wurde," and found a small increase of reaction time in those cases in which the reaction stimulus was still actually given. J. McKeen Cattell, Psychometrische Untersuchungen, *Wundt's Phil. Stud.*, 1886, 334. In our case the subjects did not know that the reactions at the beginning were practice ones, and were thus ignorant as to when the main Experiment began, as well as of the fact that a cessation of the 'catch' cards had then set in. The shortness of the series of main reactions prevented them from discovering or prematurely guessing this for themselves.

[22]In a few cases premature releases of the reaction key took place after reaction on the critical card had occurred. Such cases are not altogether without significance for the interpretation of our results. Only the introspective protocols can be given for them and are indicated below with an asterisk. The subjects concerned were additional to the 34 previously mentioned.

Experiment VII

The experimental procedure was, in general, the same in Experiment VII as in VI, with the exception that twelve letters now appeared on each card. Since the letters were of the same size as those used in Experiment VI they covered a correspondingly longer line.

Of the two sets of stimulus cards used (each approximately with the same number of subjects) one had its letters in blue, the critical letter (B) being red; the other had its letters in red with the critical letter (B) in blue. This critical (reaction) letter varied in irregular succession on the ordinary cards between the positions 1 to 12 on the cards, but came seventh as critical letter on the last card of each set. In this Experiment the subjects were explicitly instructed to pass from the letters on their left to those on their right in looking for the stimulus to reach. It was feared that, with a series of letters of such length, important and uncontrollable variations would otherwise take place, for the different cards, in the subjects' methods of search and thus, also, in the reaction times.

The Experiment was carried out with nineteen subjects of whom the reaction times are given below.[23] All the subjects were again ignorant of the special aim of the Experiment and of the fact that a change in the colour of the reaction letter would take place. With one exception they belonged to the (t) class of subjects.

Experiment VIII

In this case, too, the general procedure was the same as in Experiment VI. Now, however, in the case of the critical card, all the letters had been changed to another colour, the reaction letter alone retaining the same colour as all the letters had had on the previous cards of a set. The reaction letter on the critical card was, therefore, in this Experiment, heterogeneous in colour only with regard to the other letters appearing on the critical card, these other letters being heterogeneous in colour with the letters on the preceding cards.

Two sets of cards were used, each card having four letters similarily made and spaced as those used in Experiment VI. In the case of one set the letters on the ordinary cards were all blue, with a change to red on the part of three letters on the critical card, the reaction letter (K) coming fourth from the left on this card. The other set consisted of red letters, with a change to blue on the part of three of them on the critical card, the reaction letter (B) coming second from the left on this card. As in the case of Experiments VI and VII the same letter occurred as reaction letter on all the cards of a set.

Ten fresh subjects took part in this Experiment. All were students at the University and all were, again, ignorant of the aim of the Experiment and of the fact that any change of colour would take place.

[23] As in the case of Experiment VI there were a few additional cases in which premature releases of the reaction key took place after reaction. Here, too, use could, thus, only be made of the introspections.

The Quantitative Results of Experiments VI to VIII

Table 5 refers to Experiment VI. In the first column is given the designation of the subjects. Column 3 gives the reaction time of the subject on the reaction letter on an ordinary card. This reaction time is that obtained for the reaction occurring nearest in serial order to the reaction on the critical card, subject to the restriction that the position of the reaction letter on both ordinary and critical card was the same.[24] Column 2 gives this position.

TABLE 5

Experiment VI

Col. 1	Col. 2	Col. 3	Col. 4	Col. 1	Col. 2	Col. 3	Col. 4
Subj.	Place	Ord. Cd.	Crit. Cd.	Subj.	Place	Ord. Cd.	Crit. Cd.
Gry.	1	474	602	Lb. (t)	3	458	530
v.D.	1	337	740	Hum. (t)	3	422	557
Dr. (t)	1	458	375	Ded.	3	370	312
Cel.	1	564	769	Z. (t)	3	901	481
Btd.	1	277	301	Oost. (t)	3	452	367
Ab.	2	342	267	Bt.	3	416	720
Eb.	2	342	609	v. W.	3	449	387
C.	2	366	126	Ben.	3	415	406
De.	2	476	715	Ahl.	3	356	387
B.	2	199	289	Alb.	3	298	514
v. H. (t)	2	305	523	Dej. (t)	3	451	442
le. R. (t)	2	539	1167	vdMe.	4	715	481
A.	2	471	239	Kir.	4	648	559
Grn .	2	495	736	Klp. (t)	4	471	552
duT. (t)	2	459	1088	Nel.	4	394	1272
D.	3	717	1646	Str. (t)	4	497	997
Kn. (t)	3	378	730	Pp.	4	407	1284

Calculating from the table we find that the mean reaction times (to the nearest sigma) on the reaction letter when ordinary and when critical to be the following (the time for the ordinary reaction coming first in all cases) :—

Place 1:—422 and 557; place 2:—399 and 576; place 3:—468 and 575; place 4:—522 and 857.5.[25] The total mean reaction times for the ordinary and critical times given in the table are, respectively 451 and 623, this being an increase for the total mean critical reaction time of 36% of the total mean ordinary reaction time. There is, thus, a clear average increase in the case of the critical reaction time as compared with the ordinary reaction time. The table shows, moreover, that in 23 of the 34 cases the critical reaction times are longer than the ordinary reaction times. The unexpected change in the colour of the reaction letter, when occurring on the

[24]The comparison is carried out in this way in the cases of Experiments VI to VIII in order to eliminate possible practice effects.

[25]The tendency for the times to increase as the places of the reaction letter shifts from the left to the right of the subjects is obviously due to their attending, on the whole, first to the letters which lie more to the left.

critical card, has, thus, led, on the whole, to an undoubted lengthening of the reaction times. At the same time there is a large minority of cases (over 30%) which do not follow this rule. This indicates that the change may have favourable as well as unfavourable effects on the speed of reaction. This is reflected in the mean variations of the ordinary and critical reaction times given in the table. In the case of the ordinary reaction times the mean variation is 88 sigma (i. e., 19% of the mean ordinary reaction times); and in the case of the critical reaction times the mean variation is 259 sigma (i. e., 42%of the mean critical reaction time), the mean variation being given to the nearest sigma in both cases.[26] We shall find this result corroborated by those of Experiments VII and VIII, and that is of some importance for the argument to be developed at a later stage.

Table 6 gives the main quantitative results from Experiment VII. In the first column is given the subject's designation; in the second his reaction time on the ordinary card with the reaction letter on the 7th place from the left; and in the third column his reaction time on the critical card, where the reaction letter also occurred in the 7th place. The ordinary card taken for purposes of comparison is again that one coming nearest in the serial order, in which the cards were exposed, to the critical card.

TABLE 6

EXPERIMENT VII

Col. 1	Col. 2	Col. 3	Col. 1	Col. 2	Col. 3
Subj.	Ord. Cd.	Crit. Cd.	Subj.	Ord. Cd.	Crit. Cd.
le. R. (t)	1159	1461	deV. (t)	978	1485
Hum. (t)	966	845	Mar. (t)	822	735
Bot. (t)	959	1050	Bo.	1322	936
Kah. (t)	611	1045	Od. (t)	1108	1693
Vis. (t)	777	690	Cn. (t)	974	829
Dej. (t)	1255	1132	v. R. (t)	734	779
Lb. (t)	591	1174	laG. (t)	1398	1285
Pien. (t)	867	948	Bru. (t)	1093	880
v. H. (t)	1072	1787	Mur. (t)	1185	952
Lo. (t)	958	384			

The table shows that with 9 subjects the reaction took longer in the critical case, and that it is shorter in the critical case with 10 subjects. Among the cases in which it is longer there are, however, several of somewhat exceptional length; the mean reaction time of all the subjects becomes longer, for this reason, in the case of the critical card. The mean critical reaction time is 1057 (to the nearest sigma), and that for the ordinary card is 991 (to the nearest sigma). This gives a difference of 6.7% of the latter time. Although there is this amount of agreement with the result obtained in

[26]It is true, of course, that one would have obtained a similar increase of the mean variation of the critical reaction times if *all* of these had shown increases of sufficient irregularity.

Experiment VI, the mean ordinary and critical times approach more closely to each other, both absolutely and relatively, than was the case in the previous Experiment. A variety of causes unfavourable to quick reaction, and undoubtedly present in Experiments VI and VII will be adduced later. In the meantime, however, the reaction times alone lead to the conclusion that such unfavourable factors are more nearly counterbalanced, on the average, in Experiment VII by those favouring quick reaction.

Furthermore, in calculating, from the table, the mean variations of the ordinary and critical reaction times from their own means, we find a difference of the same nature as that obtained in Experiment VI. These mean variations are, in the present case, to the nearest sigma, 176 (17.8%) for the ordinary reaction times, and 273 (26.1%) for the critical reaction times.[27]

Finally, since the mean ordinary and the mean critical reaction times, in the present case, show only a relatively small difference, we are, now, more clearly justified in concluding from the difference in the mean variations, that the lengthening or shortening of the critical reactions is due to the relative preponderance in the case of the different subjects, at reacting on the critical cards, of either factors favourable, or of factors unfavourable, to quick reacting. This result, too, is of importance for the discussion of our main problem at a later stage.

Table 7 gives the main quantitative results obtained from Experiment VIII. In the first column we have, again, the designation of the subject; in the fourth column we have the subject's reaction time in sigma on the critical card; in the third column we have his reaction time on the ordinary card, with the reaction letter coming in the same place as on the critical card, and coming nearest the critical card in the serial order in which the cards were shown.

[27]The difference is, both absolutely and relatively, smaller than was the case in Experiment VI. This we hold to be due to the following:— As mentioned above, the subjects were explicitly instructed, in the case of Experiment VII, to examine the letters in a particular order, i. e., to pass over them from the left to the right. As we have seen, the tendency to attend to the letters in this order was also present with the subjects in Experiment VII; still, the explicit instructions in the latter case would tend to cause greater agreement in the methods of search in the case of both the ordinary and the critical reactions, and thus to equalize the two mean reaction times; and so, also, the two mean variations for these times. In addition, the longer line of letters in Experiment VII, with the reaction letter coming in the 7th place, would prevent this letter from falling in or so near the point of clearest vision when the card was exposed (since the subjects were instructed to fixate the letter on the extreme left first of all). This would, naturally, increase the necessity for a systematic examination of the line of letters before the reaction letter drew attention.

TABLE 7
EXPERIMENT VIII

Col. 1	Col. 2	Col. 3	Col. 4	Col. 1	Col. 2	Col. 3	Col. 4
Subj.	Place	Ord. Cd.	Crit. Cd.	Subj.	Place	Ord. Cd.	Crit. Cd.
LR.	2	462	356	Lb.	4	374	646
vWk.	2	442	468	Dan.	4	542	507
Al. (t)	2	469	534	vN.	4	366	435
Pl. (t)	2	351	404	Bh.	4	357	505
Sn. (t)	2	537	389	Ag.	4	615	268

In the present Experiment the reaction letter on the critical card was heterogeneous in colour only with the other letters appearing on the same card, but homogeneous in colour with all the letters on the preceding cards. We find from the table that there are six cases in which the critical reaction time is longer than the ordinary reaction time; and four cases in which it is shorter. The mean ordinary and critical reaction times calculated from the table show practically no difference. To the nearest sigma they are 451.5 in the case of the ordinary cards, and 451 in the case of the critical cards. This is a difference of almost 0%. We are thus led to the following conclusions:—

Since the mean of the ordinary and the mean of the critical reaction times in Experiment VIII practically coincide, the opposing factors favouring quicker or slower critical reactions very nearly counterbalanced each other completely *on the average*. This conclusion would not be essentially affected even if we hold, as we do, that further experiments may have given a slightly larger difference between the means. This result could arise in two ways:—either (1) considerable deviations from the mean of the *ordinary* reaction times occur among the *critical* reaction times, but those falling in this way above and below the ordinary mean counterbalance each other. We would then have the kind of result obtained in Experiment VII, where such deviations occurred, but where the two means did not lie far apart; or (2) such considerable deviations did not occur. This is the case in Experiment VIII, as an examination of Table 7 and a comparison of the mean variations of the ordinary and critical reaction times shows. The mean variation for the ordinary reactions, as calculated from Table 7, is 73.3 sigma, i. e., 16.2% of the mean ordinary reaction time. In the case of the critical reactions it is 89.1 sigma, i. e., 19.8% of the critical (and also of the ordinary) mean reaction time. From this it is clear that the *opposing factors tending to preponderate in an alternating manner* in the cases of Experiment VII, and even in Experiment VI, no longer occur in this way in Experiment VIII, or, at most, to only a slight degree.

The Introspective Results of Experiments VI, VII, and VIII and the Treatment of the Problem Arising from Our Results

In view of the results obtained in Experiments I to V, we might have expected a general tendency towards a shortening of the critical times in Experiments VI and VII. That is, we might have argued that the reaction letter on the critical card should, in these cases, have attracted attention more easily on account of the heterogeneity of its colour with that of the other letters on the preceding and on the critical card. In the case of Experiment VIII the matter would have been rather more complicated. According to the same line of argument the reaction letter on the critical card should tend to attract attention because of the heterogeneity of its colour with the other letters on the critical card. On the other hand, these other letters would tend to draw attention because of their heterogeneity in colour with the letters on the preceding cards, and so delay attention being given the reaction letter; this should then tend to lengthen the critical reaction time.

The study of the reaction times in Experiments VI and VII have, however, already led to the conclusion that opposing factors (or groups of factors) were present in them; quick critical reactions being favoured by the one kind, and slow critical reactions by the other. The introspective evidence to be adduced is in full agreement with this conclusion.

It is clear, firstly, that a certain amount of 'adaptation' of attention and reaction is needed for the reaction on the critical card to take place. This is made necessary by the fact that, whereas the subject expected or was accustomed to recognise and react on a stimulus having certain characteristics, he has unexpectedly to recognise and react on the stimulus where a change has taken place in its characteristics. Recognition and reaction tend to be delayed under these circumstances.[28]

[28]Grassi obtained this result for the reaction times in experiments where different parts of the body were stimulated by touch and where the stimulus was applied to another part of the body than that the subject was prepared for. The conclusion is "dass es auf die Reaktionszeit wirklich Einflusse hat, wenn die Einstellung oder Richtung der Aufmerksamkeit, die auf einen gegebenen Reizungspunkt eingestellt zu sein gewohnt war, aug einmal geändert wird." E. Grassi, Einfache Reaktionszeit und Einstellung der Aufmerksamkeit, *Zsch. f. Psychol.*, **60**, 1911, 72. Mann communicates an analogous observation ". . . . die Lehrer oft machen können. Wenn ein Lehrer z. B. in einer Lateinstunde plötzlich eine mathematische Frage an die Klasse stellt oder sogar nur eine Frage, die sich auf ein anderes sprachliches Fach—vielleicht deutsche Literatur—bezieht, so findet er wohl: selbst Schüler die die Frage, fiele sie in der 'richtigen' (mathematischen oder deutschen) Stunde, schnell und gut beantworten würden, versagen auf einmal völlig . . . . Der Umstand, dass

So we find that, notwithstanding the fact that attention may be drawn to the critical reaction letter appearing unexpectedly in a new colour, there is, also, evidence of an increase in the difficulty in *recognising* it as the letter to be reacted on. Compare, e. g., with regard to this point protocols 33, 35, and 39, as well as the following:—[29]

11. v. D. VI (337-740) "I was not certain at first that it was M. Then I saw that it was M, and now I perceived the M more clearly than in the other cases. I found the change a little peculiar."
12. le. R. (t) VII (1158-1461) "I looked at the first letter (sc. on the line) and saw that it was not B. So also with the second. Then I proceeded fairly quickly. When I got to B it took me a moment to comprehend that it was B. Only after I had decided that it was B could I react."
13. de V. (t) VII (978-1485) "I saw the red letter at once, but did not recognise that it was B. My eyes went on a little further, and then came back to it at once."
14. Bo. VII (1322-936) "I was slightly put out by the colour. I looked sharply to see whether it was B when I got to him."[30]

A further factor, causing a lengthening of the reaction time on the critical card, consists of the arousal of doubt in the mind of the subject, after he has recognised the reaction letter, as to whether he should react on it or not. A loss of the automatic character of the reaction thus takes place, so that special decisions to react may become necessary. The presence of such factors does not, of course, imply that the total critical reaction time must be longer than the total ordinary reaction time for any particular case, since counterbalancing factors may be present. A loss of the automatic character of the reaction in the critical case sometimes also shows itself in a premature release of the reaction key after the subject has pressed it down; as a result of this, time-readings, in such cases, were often impossible. The following protocols illustrate the effects here mentioned:—

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jetzt 'Latein ist,' bedeutet für die Masse der Schüler eine ganz bestimmte Einstellung . . . . Fällt nun die 'unerwartete' Frage, so passt sie eben nicht hinein in die psychische Verfassung der Schüler . . . ." A. Mann, Zur Psychologie und Psychographie der Aufmerksamkeit, *Zsch. f. Ang. Psychol.*, 9, 1915, 415. The nature of the attentional adaptation required from the subjects in our Experiments forms one of the subjects of discussion in the sequence.

[29]The Roman number after the designation of the subject indicates the number of the Experiment. The reaction times between the brackets are those from the tables and are repeated here for convenience of reference. The time for the reaction on the critical card comes second in each case. The number before the designation of the subject is the number given the protocol in this article. The colours and letters mentioned in the protocols refer to the reaction letters on the critical cards, unless otherwise indicated.

[30]Henning obtained similar results with regard to difficulty of recognising an unexpectedly heterogeneous element. Cp. note 3.

15. Pp. VI (no timing*) "I saw B and did not know whether I should react. I then thought that E had told me to react on B, and that it need not necessarily be red B."

16. Mar. (t) VII (822-735) "The B was red instead of blue as ordinarily. I asked myself whether E had not perhaps said that I should react on a blue B only. This idea, however, came only after reaction." (The protocol merely illustrates the arousal of the doubt and not the lengthening of the reaction time as result.)

17. Hum. (t) VI (966-845) "The first thing that struck me was that the colour (sc. of the reaction letter) was blue. Then I thought that E had instructed me to react on B, and this was B, and that I should thus react."

Further evidence to the same effect is given in protocol 23 and evidence of the disturbance of the automatic nature of the reaction by protocols 33 and 34.

We furthermore find elements of surprise occurring in the reaction period in connection with the changed nature of the critical card or letter. Compare, e. g., with regard to this, protocols 46, 53, and 54. The element of surprise thus intruding in the reaction period, must also have tended to lengthen the critical reaction time, in part (possibly), by increasing the difficulty of recognition.[31]

A study of a number of protocols to be adduced shows quite clearly that the factors which have up to now been shown to have been present and acting in the direction of lengthening the critical reaction times, were by no means the only ones. There is especially one factor, of fundamental importance, which has still to be treated. The characteristics of this factor are of such a nature that it seems—on first sight, at any rate—to stand in complete contradiction to the results obtained in Experiments I to V with regard to the validity of the laws of *Distinktionsbeachtung* und *Widerstreitsbeachtung*. There are, moreover, phenomena other than those found in our Experiments, which also do not follow these laws. In order to develop our view as to the nature and significance of these exceptions, we have found it convenient to begin with a presentation of those not found in our own Experiments. All the exceptions are characterised by the fact that, so far from heterogeneity between a content, as anticipated and as actually presented, being a factor tending to draw attention to the presented content or to the way in which it differs from what is anticipated, we find that the heterogeneity

[31]Grassi found surprise to have the effect of increasing the time of reaction. *Op. cit.*, 68. The surprise itself may be looked upon as a symptom of the increased difficulty of recognising the object in its unexpected form; cp. Wundt: " . . . . bei der Überraschung . . . . bei zufälligen Eintritt eines ungewohnten Eindrucks . . . .indem hier weder die Adaptationseinrichtungen in den Sinnesorganen noch die unmittelbar dem Bewusstsein disponiblen und eventuell in ihm in oszillierenden Bewegung enthaltenen Erinnerungselemente dem Eindruck entsprechen, braucht dieser längere Zeit zu seiner Auffassung . . . . " *Op. cit.*, 321.

acts in the direction of such content or difference not being attended to at all.

The following is the first class of case in which the effect on attention required by the laws does not take place, notwithstanding the fact that the conditions for this effect, as stated in the laws, are realised in them. The cases are, then, characterised, in the first place, by the fact that the difference is not perceived, when the object as presented differs from our expectation of it, or from our previous or immediately preceding experience of it or of similar contents. The cases are, secondly, characterised by the fact that such object, is *perceived* as being in *agreement* with previous or immediately preceding experience or expectation. In other words the perception, as compared with the object itself, shows a process of 'shift' (as we may call it) in the direction of previous contents. This 'shift' occurs, firstly, where the memory residuals for contents, similar to the one being presented, are in a relatively high degree of preparedness, without, however, any conscious expectation having been present; the 'shift' occurs, secondly, where there was an expectation for a content similar to, but differing in some respect from, the one actually being presented. Observations in daily life as well as the laboratory experiment (especially the tachistoscopic) provide demonstrations of this fact.[32]

[32]Examples from daily life are given, e. g., by Ebbinghaus-Dürr *Grund d. Psychol.*, **2**, 1913, 10-11. J. McKeen Cattell, Ueber die Trägheit der Netzhaut und des Sehzentrums, *Phil. Stud.*, **3**, 1886, 116-7, found that certain letters were easily mistaken for others in tachistoscopic perception. W. B. Pillsbury, A study of apperception, *Amer. J. Psychol.*, 8, 1897, made use, in such experiments, of words containing misprints, and found many cases in which the words were read as if correctly spelled (348, 350, 351), or where the impression of the word was influenced by preceding words used (359-60), or by words occurring in the previous experimental hour (362), or by the effect of words called out aloud at exposition (361). B. Erdmann and R. Dodge, *Psychologische Untersuchungen über das Lesen.*, 1898, 180-181, state, as the result of their introspection on correct as well as incorrect readings in tachistoscopic experiments, that "Sie (sc. die optischen Residuen) wirken nicht als Erinnerungsbilder irgendwelcher Art, seien dies direkte Erinnerungen an einer der früheren Wahrnehmungen, seien es als abstrakte Allegemein-vorstellungen des ihnen allen gemeinsamen. Eine solche selbständige Reproduktion könnte nur angenommen werden, wenn irgendeine Spur derartiger Bilder als konstante Vor-oder Begleiterscheinung der Wahrnehmung aufzufinden wäre . . . . Diese (sc. hier vorliegende) apperzeptive Reproduktion ist vielmehr eine *unselbständige*, sie ist dadurch charakterisirt, dass die erregten Residuen nicht selbständige Komponenten unseres Wahrnehmungsinhaltes bilden, sondern dass sie als apperzeptive Reizkomponenten zu dem Wahrnehmungsganzen *verschmelzen*. Für diese *apperzeptive Verschmelzung* . . . . bürgt das Moment des Unmittelbaren, dass wir in allen Fällen zu konstatiren hatten, wo das Wort tatsächlich oder scheinbar deutlich erkannt war." (Note:—under the 'scheinbar deutlich erkannt' we must understand the 'Verkennungen.') Further results substantiating the statement in the text

We may express as follows, in abstract symbols, the process taking place in cases of this type:—There exists, on the one hand, either the residuals or the expectation for a content with the characteristics AB. Should an object having these characteristics be now presented, so that central and peripheral excitations work in the same direction, then these characteristics will attain to a higher degree of attention,

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were obtained by J. Zeitler, Tachistoscopische Untersuchungen über das Lesen, *Phil. Stud.*, **16**, 1900, in his 'Suggestions-Assimilation' experiments:—"Die Versuche gingen so vor sich, dass soerst die richtigen Wortbilder exponirt wurden, die der Beobachter geläufig las. Dabei war er nicht beauftragt, sie besonders einzuprägen, da jede Voreingenommenheit der Versuchsperson ausgeschlossen bleiben musste. Die falschen Wortbilder wurde hierauf in der Weise exponirt, dass sie dem ihnen entsprechenden Wortbild in einem Abstand von 3 Expositionen folgten . . . . Das vorhergehende Wortbild wirkte als Suggestion zur Beeinflussung des ihm folgenden falschen Wortbildes . . . . In einer zweiten Versuchsreihe folgte das falsche Objekt in einer unmittelbaren Exposition dem primären Eindruck. Durch lautes zurufen des Wortes vor der Exposition wurde der Eindruck noch verstärkt." Zeitler says of the mistakes in reading thus arising that "Der ganze Vorgang trägt deutlich den Charakter der Illusion, indem sich das suggerirte subjektive Wortbild über die objektive veränderte schiebt und es in seinem Sinne umwandelt, während es doch eigentlich in seinem objektiven Bestande gelesen werden sollte." (450-2) In view of the findings of Erdmann and Dodge, quoted above, we add that Zeitler's hypothesis that 'das reproducirte Wortbild schiebt sich über das aufgefasste' is, at the very least, not true for all cases if his statement is meant to imply that the correct word is first of all correctly 'aufgefasst' even in the sense of a chronologically preceding perception. A condition that must be realised, according to Zeitler, for these illusions to arise, is that ". . . trotz der Verstümmelung des Wortbildes (i. e., of the succeeding word) die Wortform erhalten geblieben ist, was dadurch erreicht wird, dass die Buchstaben nur innerhalb ihrer Aehnlichkeitsreihe verändert werden." Zeitler obtained similar results in other ways; so, e. g., subjects did not notice that letters had been left out in the word exposed (454 f.), and also found that the expectation that mistakes would occur in the words exposed, led to such mistakes being 'seen,' even when they were not actually present (456). C. F. Wiegandt, Untersuchungen über die Bedeutung der Gestaltqualität für die Erkennung von Wörtern, *Zsch. f. Psychol.*, **48**, 1908, 168 ff., used the method of exposing words for longer times, but at considerable, though differing, distances from his subjects, and also found that words similar to each other were mistaken for each other. (The polemical point as to the degree to which 'Gesamtform' or 'determinierende Buchstaben' are responsible for the effect does not concern us here.) F. Grossart, Das Tachistoskopische Verlesen, *Arch. f. g. Psychol.*, **41**, 1921, in following the method of first showing the word, or other combination of letters, at considerable distances from the subject, and of then decreasing this distance for the same object, found that there was a tendency with all the subjects to read less familiar words as familiar words "solange noch nicht bestimmt erkannte Einzelheiten eine Hemmung dafür bildeten." (146.) "An Stelle des ungeläufigen wurde ein im visuellen Wortbild ähnliches geläufiges Wort mit maximaler Sicherheit gelesen." (149.) An important factor for causing mistakes in the readings, "liegt in dem Festhalten des reproduzirten Worten (sc. from previous expositions) das als Erwartungsvorstellung in weiterem Sinne die nächste Lesung beeinflusst." (181.)

than if, e.g., only one of these conditions of attention had been present in the same strength. These characteristics will then also come to attention more quickly.[33] The fact of the reaction letter on the card in Experiment VIII occurring in the same colour as that in which it would be anticipated must, according to the above, have assisted in bringing it to attention more quickly than if only the peripheral excitation were present. If, now, the two excitations do not so agree (such as would be the case, if, e. g., the expectation were for an object AB, and the object presented had the characteristics AC), then the resulting effect in consciousness would de-

[33]So, e. g., Ries found that a number (up to 14) of tachistoscopic expositions were sometimes necessary before his subjects noticed that the marks on the exposition cards were no longer neutral (black) but had received other colours. The subjects had not expected the change and the effect must be ascribed to the corresponding weakness of the residuals. G. Ries, *Zsch. f. Psychol.*, 88, 1922, 161 f. Cp. also the present writer's, An examination of Külpe's experiments on abstraction, *Amer. J. Psychol.*, 34, 1925, 324, where it was shown that Külpe is correct, in principle, in holding that "Die Abstraktion im Sinne des Hervorhebens gewisser Teilinhalte, die *positive* Abstraktion, gelingt . . am besten, wo vorher eine Präoccupation des Bewusstseins, eine Prädisposition für diese Teilinhalte gegeben oder gesetzt war." O. Külpe, Versuche über Abstraktion, *Bericht über den I. Kongress für experimentelle Psychologie,* 1904, 57-58. Cp. further note 3 on observations of Henning. The statement in the text is made according to the Law of prior entry, according to which "The stimulus for which we are predisposed requires less time than a like stimulus, for which we are unprepared, to produce its full conscious effect," this law being applied for the explanation of the negative displacement in complication experiments. E. B. Titchener, *Lectures,* etc., 251. Attention may here be drawn to one limitation to the validity of the statement that the previous occurrence in consciousness of the same content is favourable to its coming to clear attention on a later occasion. The limitation consists, according to Ranschburg, of the inhibitory attentional effect on a content which has shortly before been preceded by, or is accompanied by one having wholly or partly identical characteristics. P. Ranschburg, Über die Wechselwirkung gleichzeitiger Reize im Nervensystem und in der Seele, *Zsch. f. Psychol.*, **66**, 1913, 161, and **67**, 1913, 22. Possibly we have to accept, with Henning, the existence of a refractory phase of very short duration in the residual in order to explain this phenomenon. According to Henning it does not occur when the whole series of elements is perceived simultaneously as a whole. H. Henning, *Die Aufmerksamkeit,* 1925, 154-5. It should be added that Ranschburg had, on the basis of the inhibitory effect observed by him, formulated the generalisation that "Das Entstehen und Bestehen einer Wahrnehmung oder reproduzierten Vorstellung V im Bewusstsein hängt nicht nur von ihrem gegenwärtigen psychophysiologischen Eigenschaften ab, sondern auch in bedeutendem Grade von der Qualität der kurz vorher im Bewusstsein vorhanden gewesenen, sowie der bald hernach auftauchenden Inhalte ab: Sind diese der Vorstellung V heterogen, so wird sie zu einem autonomen, reproduziblen Inhalte des Bewusstseins. Sind sie ihr ähnlich so verschmelzen die identischen Teile der Vorstellungen V, V_2, etc., zu einem Ganzen . . . ." Über die Bedeutung der Aehnlichkeit für das Erlernen, Behalten und die Reproduktion, *Bericht über den I. Kongress für experimentellen Psychologie,* 1904, **54.**

pend, in the first place, on the relative effectiveness of the two excitations. If the peripheral excitation corresponding to the characteristic C, of the object, is relatively weak, and the central excitation for the characteristic B is relatively strong, then B will take the place of C in the perception of the presented object. Even where AB has not been in consciousness, before presentation of AC, in the form of an expectation, a sufficiently high degree of preparedness of the corresponding residuals would also bring about the 'shift' in perception. In addition, the further excitation accruing to A from that part of the peripheral stimulus corresponding to it, would, because of B's association with A, also increase the excitation of the residual for B, thus increasing the causes favouring a 'shift' in perception in the direction of AB. The 'shift' is, thus, a process of dependent apperceptive reproduction.[34] Under these circumstances, then, a difference between the content presented and similar contents of previous or immediately foregoing experience, or with the content of expectation, does not draw attention.

There is, however, a second effect, which takes place especially when there has been a preceding *expectation* of AB, and where AC is now actually presented. This effect consists of the fact that the content AC, when now presented, is *overlooked*. Neither does a 'shift' in perception in the direction of AB take place, nor does AC (or its difference from AB) draw attention, at least for some time. The presence of this negative effect, which must have played an important role in lengthening the critical reaction times in Experiments VI and VII, is testified to by the following protocols:—

18. Str. (t) VI (497-997) ". . . . I found no difficulty, as a rule, in allowing K, on appearing, to come to awareness. There was one marked exception—the last case. I seemed not to see the reaction letter at all. There was a painful hesitant state, such as 'What on earth am I to do now?' All at once came the awareness of the colour and that this was the letter on which I had to react."

19. Bp. (t) VI (no timing*) "I did not see B at all at first. When I saw it, I was in doubt and felt ashamed at not having seen it sooner."

20. Dr. (t) VI (458-375) "I did not see the black letter at all at first. (Note:—this letter had the first place on the left of the subject.) I first noticed the green letters. Then only did my eye fall on M. Its black seemed to me at first merely to belong to the black surface enclosing the aperture."

21. Ahl. VI (356-387) "At first I almost thought that there was no L. Then I saw it coloured."

22. Pp. VI (407-1284) "When the letters appeared I did not see K at all. I saw only the blue letters. Afterwards I became conscious of—'Why, there is a red K standing there.' "

23. Bs. VI (no timing*) "I did not see the B at first for a long time, and when I saw it I was in doubt as to whether I should react."

24. Lb. (t) VI (458-530) "I looked through the letters quickly, and did

[34]Erdmann's 'unselbständige apperzeptive Reproduktion'. Cp. note 31.

not at first see B, though I noticed that the card was somehow different in one place. Then I looked in that direction and saw B in blue."

25. Gbe. VI (no timing*) "I looked at the card and at first thought that there was no K on it. Then I saw the K and became conscious of the fact that its colour was different from that of the other letters."

26. v. R. VI (no timing*) "The difference in colour put me off. I could not see the K at first."

27. Klp. (t) VI (471-552) "My attention was directed in the first instance to the three blue letters. Then the red letter drew my attention very sharply. When I saw the red I thought it was K without being quite certain of it."

28. Nel. VI (394-1272) "I saw three of the letters clearly, but K I hardly saw at all at first."

29. Kah. (t) VII (611-1045) "I passed along to the end of the line, then I went back again and now saw B at once and noticed that it had another colour, namely red. I do not remember having passed along the whole line in previous cases without seeing the stimulus letter."

30. v. H. (t) VII (1072-1787) "My eyes went along the line for some distance without my seeing B. Then I looked back, and I saw the red B. On first looking over the letters I noticed that there was something strange . . . ."

31. Lb. (t) VII (591-1174) "I went right through the line of letters without noticing B. Then I went through the line backwards, and then I noticed B and that its colour was red, even before reacting. On going through the line the first time I had noticed that there was something strange about it."

32. Bru. (t) VII (1093-880) "B did not attract my attention at first. I had to go down the row. I noticed before reacting that the colour was different. I have the impression that I hesitated a little before reacting."

According to the evidence of these protocols we conclude that the unexpected heterogeneity of the colour of the reaction letter on the critical card acted, in these and similar cases, in causing the previously defined negative attentional effect.[35] It must, thus, have acted in the direction of lengthening the reaction times in Experiments VI and VII. Furthermore, in our discussion of the reaction times in Experiment VIII, we found that the opposing factors preponderating in an alternating manner for lengthening or shortening the critical reaction times in Experiments VI and VII had practically been eliminated. We conclude that, among those eliminated, was the negative attentional effect with regard to the reaction letter on the critical card.

Further evidence as to the reality of the negative effect is forthcoming from other writers. Titchener gives the following introspective account of the way in which this effect occurs with him as against W. Fuchs' view as to the explanation of our overlooking

[35]It may be noted that this effect is not that of a negative abstraction due to a decision, previous to exposition, not to attend to a letter in another than the usual colour. Neither can it be due to a habit not so to attend. We did, however, obtain one protocol (cp. number 51) where such abstraction took place after exposition and after the change in colour had been noticed.

things in everyday life, according to which this overlooking is due to the levelling out of their colour, by assimilation, to the colour of their surroundings. Titchener states:—"I very often 'lose' things in the tangle on my desk . . . . . I have great difficulty . . . with spectacles and desk-shears. I habitually lay these things down, after use, in a particular position; if, in the shuffle of papers and desk furniture, they are brought into other positions, I do not see them; sometimes, though I know that the object is lying before me, I am obliged to ask the assistance of another pair of eyes . . . I have now, and have had for a good many years, definite schematic visual images of these two things in their 'proper' positions. I have also a definite motor set or attitude for picking up and replacing the glasses (both hands) and for taking up the shears (right hand) . . . . what I try to do is to fit the image over something that I see in the litter before me . . . If, then, the image does not fit any visible object, or there is nothing in the visual field that discharges the motor set into overt movement, then I fail to see what I am looking for . . . Discovery may come about in various ways . . . . Oftentimes . . . . I have paused to observe image or attitude, and have dismissed the image or relaxed the attitude with the result that the object stares me recognisably in the face . . . ."[36]

We have, then, in this negative effect, a second type of case where the conditions stated in Seltz's *Gesetz der Widerstreitsbeachtung* are realised, but where the result does not correspond to that given in the law. The cases falling under this type are characterised by the fact that, notwithstanding a certain degree of correspondence between the central and the peripheral excitation, the object does not come to attention as the one sought for, at any rate not for some time, there being a corresponding delay in the reactions. This means, expressed in terms of abstract symbols, that the characteristic (s) A of the presented object also suffers attentionally. So, e. g., the mere *shape* B of the heterogeneously coloured reaction letter, in Experiments VI and VII, did not, in many cases, come so well to the forefront of consciousness; as a consequence it would be recognised less quickly as the letter to be reacted upon. In other words, the fact that the anticipation of the object as AB does not agree with the object as presented in the form AC with regard to the characteristic C, has a deleterious attentional effect on the characteristic A, with regard to which both excitations agree. The ordinary effect, in consciousness, of such agreement between central and peripheral excitations is, at the very least, delayed. We hold that the degree, to which this inhibiting, negative effect takes place, will depend, other things being equal, on the degree of prom-

[36]E. B. Titchener, The Overlooking of Familiar Objects. *Amer. J. Psychol.*, 35, 1924, 304–5.

inence in the conscious state of expectation, of the characteristic B. So, e. g., we see, from the passage quoted above from Titchener, that the negative attentional effect disappears, and the object sought for is perceived, when the visual image of, but not agreeing with, the object, in all respects, is dismissed. In some of our own protocols the decrease in the vividness of the characteristics as anticipated is indicated by the fact of other processes becoming momentarily prominent. These, more especially, take the form of the consciousness of failure. Compare e. g., 18, 22, and 25. According to this view we interpret the results now more particularly being discussed of Experiments VI and VII as follows:—The subjects set to work with an expectation for the reaction letter in a particular colour. If the characteristic of the colour was sufficiently prominent in the expectation, then the reaction letter, since it was presented in a different colour on the critical card, would not at first attract attention and be recognised as the letter to be reacted on. But when, at some stage, (e. g., after a fruitless search for a reaction letter coloured as anticipated) this characteristic of the expectation should become relatively less prominent, then the cooperation between the central and the peripheral excitations would take place more efficiently with regard to the points of agreement; the shape of the letter itself would come more fully to consciousness and attention, and the letter would be recognised as the one to be reacted on.[37]

According to the view developed up to this point, the heterogeneity of the reaction letter on the critical card with its characteristics, as anticipated, would act in the direction of lengthening the reaction times. Though this was, according to the reaction times, preponderatingly the case in Experiment VI, and also in many cases in Experiment VII, it still remains true that a lengthening of this nature was often not apparent. There are, indeed, protocols, among those already adduced, which indicate that the reaction letter, in Experiments VI and VII, often did come to consciousness

[37]In Experiment VIII there was also present a disagreement between the anticipation of the colour of some of the letters on the critical card, and the colour of these letters as actually presented. Applying the principles formulated in the text to this case, we conclude that this disagreement must have been unfavourable to these letters coming to attention and being easily recognized. This result *alone* would, however, not tend to affect the critical reaction times to the same degree as when the reaction letter itself, as anticipated and presented, disagreed, since a state of clear attention and a process of clear recognition of these letters *for what they were*, would not be necessary for reaction. It would be enough if they were recognised as not being the reaction letter specified in the instructions.

This argument is not meant as to imply that additional causes might not have been present for bringing about the higher attentional state and fuller identification of the letters, other than the reaction letter, on the critical card. We shall see that such was the case.

to a marked degree *after* the 'overlooking' effect had taken place (Cp. numbers 22 and 27).[38] A considerable number of further protocols, however, show clearly that the reaction letter, in the unexpected colour, very quickly drew the subject's attention; the consequence was that a decrease of the critical reaction time, as compared with the ordinary reaction time, *tended* to occur, although disturbing factors sometimes prevented an actual decrease. The following protocols are cases illustrating this positive attentional effect in Experiments VI and VII.

33. Kn. (t) VI (378-730) "My eye was drawn to the blue immediately, but something hindered my reacting on the unaccustomed colour B had. I have the impression that I tried to make sure if it were really B. When I was quite sure that it was B, I had to decide to react first of all."

34. Bh. (t) VI (no timing*) "I let my eye pass as usual from the left to the right, looking for a red B. The blue B struck me at once on seeing it, and I decided at once to react on it, though there was a slight doubt as to whether I should really react on it."

35. Z. (t) VI (901-481) "B drew my attention at once, but I was not certain that it was B, and I looked at the other letters before returning to it."

36. v. H. (t) VI (305 523) "As soon as the shutter fell I noticed that there was something unusual. I recognised B, but, before I pressed, I had the thought that this was a special case, and that I must react quickly . . . . "

37. Dej. (t) VI (451-442) "I have the impression that B drew my attention more quickly than otherwise . . . . "

38. Oost. (t) VI (452-367) "Before the shutter fell I had the thought that I had reacted well in the last few cases, and I decided to do so again. When the shutter fell the blue B attracted my attention at once, and I reacted with a feeling of satisfaction. I had, however, seen that the other letters were red."

39. v. W. VI (449-387) "I saw the L more easily than in the earlier cases, but I have the impression that I was somewhat hindered in reacting. I had first of all to look what the green was."

40. Ben. VI (415-406) "My eye fell first of all on the L having a different colour."

41. Alb. VI (298-514) "The letter caught my eye at once."

42. vdMe. VI (715-481) "I imagine I was helped (sc. by the change in colour) because the red letter drew my attention, though I was not expecting a red K."

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[38]Cp. also, O. Messmer, Zur Psychologie des Lesens bei Kindern und Erwachsenen, *Arch. f. d. g. Psychol.*, **2**, 1904, 252. He found in his tachistoscopic experiments that "Wenn die Vp. dem Reize einmal eine bestimmte Erwartung entgegenbringt, so tut sie dem exponierten Worte immer Gewalt an, solange die Erwartungsvorstellung andauert . . . . Sowie nun ein vorher unbemerktes Element . . . . endlich die Reizschwelle auch überschreitet, wirkt dieses Faktum mit instantaner Verblüffung auf die Vp., und die neue Wahrnehmung erscheint mit unverhältnismässig grosser Deutlichkeit." It is true that the cases Messmer gives as illustrations are, in our view, partly of the nature of 'shift' and partly of the nature of 'overlooking.'

43. C. VI (336-126) "The blue B attracted my attention more quickly. I was expecting a red one."
44. B. VI (199-289) "The blue B, contrasting with the others, attracted my attention more quickly. I have the impression that there was a pause between perception and reaction, so that my reaction was hindered in this way."
45. Ab. VI (342-267) "The blue B drew my eye more quickly."
46. MSt. (t) VII (no timing*) "A soon as the shutter fell I saw the B. I did not expect to see it so quickly and in another colour, and so I did not react at once. I am accustomed, otherwise, to search for B and was startled on seeing him at once."
47. Vis. (t) VII (777-690) "I fixated on the left and at once saw the first letter. The red B then immediately drew my attention. I was conscious of the intermediary letters but not of what they were. The reaction took place automatically when I saw B."
48. Bot. (t) VII (959-1050) "I passed over from the left to the right as usual with a dark B clearly in consciousness. Suddenly I was stopped by seeing a red B."
49. Pien. (t) VII (867-948) ". . . . When the card appeared the red B at once drew my attention to it, and I decided to react on it. After reaction I found it peculiar that B was red in this case."
50. Vn. (t) VII (no timing*) "B drew my attention quickly and without looking at the other letters—except the first—I looked in its direction at once. . . . . "
51. Cn. (t) VII (974-829) "Just as the shutter fell the (sc. new) colour drew my attention, and I thought this would be the reaction letter. The idea at once came up that I would lose time if I fixated the letter with the colour at once. I did not fixate him at once, but began from the left as usual. When I got to the coloured letter I noticed that it was B."
52. v. R. (t) VII (734-779) "I began with the letters on the left, intending to pass over them at an even rate. I did not see the dark B at once. When I got near to the place where it was, my eyes seemed to fly towards it. I then perceived it at once, very clearly, and reacted at once."
53. Mur. (t) VII (1185-952) "My eye moved to the middle of the series practically at once. I noticed at once that B was blue, and the new colour surprised me. Usually I tend to jump over the letters in the middle, and to go from the first three to the last three."

There are also some protocols, relevant to the present point, from Experiment VIII.

54. vWk. VIII (442-468) "I was surprised to see that some of the letters were blue in this case. The blue drew my attention in the first instance. Then only did I see the red letter."
55. Al. (t) VIII (469-534) "I began looking for B at once. The different colour drew my attention immediately, i. e., the blue (sc. of the other letters) struck me before the red B."
56. Sc. (t) VIII (537-389) "The letters struck me as different from what they were like in other cases. Then only did I notice the B."
57. Dab. VIII (542-507) "As a consequence of the red letters I at first thought there was no K. Afterwards I saw the K. I first attended to the red letters."[38a]

[38a] There are cases in Experiment VIII where this distraction from the reaction letter on the critical card did not take place, at least not to a marked extent:—

From these results, as well as from those of Experiments I to V, it is clear, that the processes of 'shift' and of 'overlooking' (with its attendant difficulty of recognition) cannot be the only effects of disagreement between the characteristics of the object as anticipated and as presented. On the other hand, these effects must be taken into account in developing any theory as to how the positive attentional effect, to which Selz's laws refer, is caused.[39]

58. Pl. (t) VIII (351-404) "My eye fell at once on the red letter, and I saw that it was B. I did not notice the other letters at all."

59. LR. VIII (462-356) "When the shutter fell it simply came to my consciousness, that there was a B. Only after reaction did I become conscious that the other letters were blue."

60. Ag. VIII (615-263) "I at once saw the K. I think this helped me to react quickly."

We can advance two factors, probably coöperating, for cases of this kind. They would be (1) the existing, more or less definite, expectation of the reaction letter in the correct colour, and (2) the negative attentional effect of the disagreement between the colour of the non-stimulus letters as anticipated and as presented.

[39]E. B. Titchener holds, on the one hand, that the effect of congruity of a stimulus with consciousness is a determinant of attention, and states, with regard to it that " . . it is clear that the more nearly an incoming excitation coincides with an excitation already in progress, the more easily will it make its way within the nervous system, and the more dominant will it become"; he also states, on the other hand, that "Sudden stimuli impinge upon nervous elements that have recently been free from stimulation of their particular kind, that is, upon elements of a high degree of excitability; and it is also probable that the excitations which they arouse suffer less dispersion and diffusion, within the nervous system, than those set up by the gradual application of stimulus . . . . Novel stimuli, being isolated stimuli, arouse excitations which are not interfered with by others; their action is akin to that of sudden stimuli." *Textbook of Psychology,* 1916, 270-1. He furthermore quotes with acceptance the analysis of T. Lipps according to which "der Reiz des Neuen und Ungewohnten ist nichts anderes als der Reiz d. h. die Fähigkeit der Inanspruchname und der Festhaltung psychischer Kraft, die einem Vorstellungsinhalte oder Komplex von solchen zukommt, *ehe* diese Fähigkeit durch die auf Erfahrungsassociationen beruhende Tendenz der Ausgleichung und des Abflusses sich vermindert hat. Der Reiz des Neuen ist nichts anderes als der unverminderte Reiz des Objektes"; Titchener adds, however, that novelty should have a special place in the empirical classification (sc. of determinants of attention). *Lectures*, etc., 357-8.

We have the following objections to this view:—(1) It is clear that the novel stimuli in Experiments I, VI, VII, and VIII attained to a high degree of attentional effect *notwithstanding* interference by others; furthermore, so far from the novelty, in such cases as ours, being favourable for the elimination of such interfering excitations, it tends, rather, to favour their intrusion since the degree of similarity existing between the novel and the already proceeding central excitation acts in the direction (successfully or not) of causing an apperceptive 'shift.' We hope to show in the sequence that interfering excitations were present also in our other Experiments with regard to the positive attentional effect of novelty. (2) The

It is clear, furthermore, that for the positive attentional effect to take place, some causal antecedent must have come into action additional to, or in the place of, those causing the phenomena of 'shift' or of 'overlooking'; or, at least, the latter must have dropped away to some extent. The view which we present as to the difference in the causal conditions is, in short, the following:—Should there be a sufficient degree of similarity between the contents AB and AC (taking them, e. g., as being on further analysis abcd and abcf), and should the peripheral or incoming excitation for C be sufficiently weak, as compared with either, or both, of the central and peripheral excitations corresponding to A, then the phenomena of 'shift' will take place, and the characteristic C will be perceived as B as long as a change in these causal conditions does not take place. So, e. g., a word incorrectly spelled is again and again perceived and recognised as if correctly spelled if presented under conditions not favouring efficient peripheral excitation.

Again, if there is present a central excitation corresponding to the content AB, and if there is presented, at the same time, an object AC, then the discrepancy between B and C will, under a certain condition, first weaken or inhibit the cooperation of the congruous central and peripheral excitations; as when, e. g., the critical reaction letter is correctly anticipated as to shape but not as to colour. The condition referred to consists of the comparative prominence, in the state of anticipation, of the characteristic B (e. g., of the colour of the reaction letter); and the degree of inhibition will vary, other things being equal, with the degree of prominence of B. Given the necessary relative degree of prominence of B, the content AC will be 'overlooked,' i. e., neither its constituent A nor its constituent C will come to attention, nor will it be recognized as having the anticipated characteristic A (e. g., as having the shape of the letter to be reacted on). Should, now, B sufficiently lose this prominence, then, other things being still equal, the incoming excitation, in so far as it corresponds to A and to C, will overcome any degree of interference still existing.

It may happen, however, that the 'overlooking' effect does not occur in a fully developed form. Should the dissimilarity between the central and the incoming excitations, with regard to their respective contents, be too great to permit of a process of 'shift,' we could still have present, in the partial incongruity of the two excitations, the causes acting, in the first place, in the direction of bringing about the 'overlooking' effect. Their action, weak though it may

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view does not take account of, and so does not explain, why it is that the novel characteristics of a content, in such cases as those that have been advanced, may tend to let the content be 'overlooked.' Other objections will be advanced in the sequence.

in some cases be, must favour a delayed coming to attention and recognition of AC. We conclude, then, that such delaying activity must have been present in all cases in our Experiments in which a presented critical content did not agree with the anticipation, and that the influence of the delaying factors had to be overcome when the critical content came to attention and recognition.

This, however, leads to a further conclusion. It is, namely, that, *after a phase of relatively full, or of more or less incipient inhibition, the delayed process of coöperation between the central and the incoming excitations in so far as congruous, and also the delayed effect of the incoming excitation for C, sets in with increased energy.* The increased degree of attention thus accruing to the content AC will then also facilitate, and so precede, the process of recognising it for what it is.[40]

So, e. g., in Experiments VI and VII, there must have been present, in general, even if only for a very short time in some cases, the inhibitory effect due to the partial incongruity of the reaction letter on the critical card as anticipated and as presented; and, mutatis mutandis, the same holds true for Experiment VIII. We have found the protocols in these Experiments indicating again and again the positive attentional effect for the critical letter and its colour.[41] So, too, in the case of Experiment I, we hold that it was the *critical*

[40]G. E. Müller, *Abriss der Psychologie*, 1924, 110, states that "Man spricht vielfach davon, dass das Neue, das Unerwartete . . . . die Aufmerksamkeit auf sich ziehe. Hierbei handelt es sich im Grunde nicht sowohl um solche Faktoren, die einem Objekte die Aufmerksamkeit *zuwenden* lassen (um z. B. als etwas neues einen besonderen Eindruck zu machen, muss ein Objekt doch bereits von der Aufmerksamkeit erfasst sein), als vielmehr um solche, welche die Aufmerksamkeit und das Denken bei einem Objekte *verweilen* lassen." As against Müller, and in agreement with the view developed in the text, we find ourselves in agreement with Henning where he advances the following against Müller:—"Der Reiz des Neuen, Überraschung durch Unerwartetes, Abwechselung und Kontrast zu bisherigen Erlebnissen, das Ausserordentliche, Fremdartige, Wunderbare und Seltsame wirken jedoch keineswegs bloss dahin die Beachtung bei sich verweilen zu lassen, sondern die Aufmerksamkeit wird ganz *primär* wie von einem faszinierenden Magneten angezogen . . . . Ehe noch die Auffassung des Neuen oder Fremdartigen durchdrang und lange bevor sie beendet ist, spüren wir eine zunehmende Fesselung, ein faszinierendes Angezogenwerden . . ." H. Henning, *Die Aufmerksamkeit*, 118.

[41]It is, of course, testified to also by the tendency for the reaction times in Experiments VI and VII to be shortened when this effect took place near the beginning. (Cp. protocols 35, 38, 39, 42, 43, 47, 53.) The fact that this shortening is not always apparent in such cases, must be ascribed to the presence of other causes tending to lengthen the total reaction times of the individuals concerned. The further question as to why, notwithstanding the delay that the view stated in the text demands, the critical reaction times in these Experiments should tend to be shortened, and not lengthened, as total reaction times, will be discussed later.

letter as coloured which attained to a relatively high degree of attention with the subjects. We hope to show, in the sequence, that also our other experimental results are in agreement with this view. In so far as the inhibiting effect of existing residuals or central excitations, incongruous with incoming excitation for C, falls away in the manner stated, it is true that "Der Reiz des Neuen ist . . . . der unverminderte Reiz des Objektes." Since, however, as our Experiments show the positive attentional effect of the unexpected novelty is greater, when it occurs, than where the incoming excitation is assisted by a congruent central excitation; and since the such cooperation is a more powerful determinant of attention than the incoming excitation alone, we are led to the conclusion that "Der Reiz des Neuen" is not *merely* "der verminderte Reiz des Objektes."[42]

We thus come to a conclusion which is in agreement with a view already formulated by Henning according to whom the primary positive attentional effect of the new and unexpected is caused by the fact that the incoming excitation comes into conflict with the only partially similar residuals brought into activity by its influence; this brings about, in its turn, an accumulation of energy in a circumscribed locality; and this, again, an increased attentional effect for the new.[43]

Under the condition, then, of a preceding (and possibly short-lived) phase of delayed cooperation between the central and incoming excitations, and of a delayed effectiveness of the incoming excitation in so far as incongruous, the characteristics A and C of the object presented will eventually be able to come to specially clear attention. In this way the conditions will be improved for the difference between the object as anticipated and presented coming to attention; in so far, namely, as this depends on the degree of attention attained to by C itself. We arrive, in this way, to a fuller formulation

[42]Cp. note 39.

[43]". . . . für das Neue und Fremdartige steht noch kein tief gebahntes Residuensystem zur Verfügung . . . . in Ermangelung zugehöriger Residuenkomplexe, müssen die nächst ähnlichen eintreten, allein sie geraten mit der vorliegenden Reizwirkung in *Widerstreit*. . . . die Sinneserregung neuer und fremdartiger Reize (findet) kein entsprechendes Residuensystem vor, das nächst ähnliche dient als Ersatzmann, hat aber hier abweichende Schaltungen, und es resultiert ein Shock. Hemmung bedeutet hier nicht Vernichtung, sondern die Sinneserrugung ist zeitweilig nur ausserstance abzufliessen, sie *staut* sich, solange die Residuenschaltung noch widerstrebt. Erregungsstauung ist eine Ansammlung von Energie an dieser Stelle, und eine Anhäufung der Erregung auf eng umgrenztem Bezirk hat wieder eine Beachtungssteigerung zur Folge. So bedeutet die Konzentrierung der Energie durch Stauung eine Konzentrierung des zugehörigen Erlebnisses. In diesem Momenten wurzelt die attentionelle Wirksamkeit des Neuen, Fremden, Seltsamen und Kontrastierenden." H. Henning, *Die Aufmerksamkeit,* 118-119.

of the conditions under which the attentional effect arises referred to in Selz's *Gesetz der Widerstreitsbeachtung.*

The further question now arises as to the possibility of explaining, on the theory here advanced, how there could be a tendency present for the critical reaction times in Experiments VI and VII to be shortened in some cases as a consequence of a *retarded* positive attentional effect? We have to take into account that there was not one (the critical) letter only presented the subject for examination, but several letters. According to the theory, while the subject's eyes are moving over the letters, a retardation of the attentional perception (and recognition) of the heterogeneously coloured reaction letter on the critical card must first of all take place. If, now, a case occurs in which the ordinary colour of the reaction letter possesses comparatively slight prominence in the conscious state of expectation, then the inhibition is overcome correspondingly more quickly. If this takes place quickly enough, e. g., before the eye has moved on to the next letter, or as long as the after-effect of a possible foveal stimulation is still strong enough, or if the indirect stimulation from the presented critical letter is sufficiently strong, then the coöperation between central and peripheral excitation can still set in before the eye has moved to a considerable distance from the letter, and the incongruous colour of the letter may also attract attention at a comparatively early stage. The 'overlooking' effect will then only take place in a more or less incipient way, and the letter soon attains to a high degree of attention. This, of course, does not yet explain a decrease of critical reaction time. The inhibitory phase may, however, already set in before the eye actually fixates the critical letter in moving in its direction, and it may already be overcome, or be in process of being overcome, before the eye actually fixates the critical letter. According to the degree in which it is overcome the heterogeneous colour attains to an increased degree of attention before the eye has reached it. More or less developed processes of coöperation between central and peripheral excitations also set in, so that the subject gets the impression or suspects that the heterogeneous content is the reaction letter being sought for. The consequence is that the subject's eyes tend to fly over the intervening letters to the letter so drawing his attention. This latter process is one which we find mentioned explicitly in some of the protocols,[44] and we consider the conclusion reasonable that the presence of such eye movements is indicated in other cases where emphasis is laid on the quickness with which attention was drawn to the heterogeneously coloured letter in Experiments VI and VII. The consequence is that the subject is assisted to shorten his reaction time. The considerable

[44]Cp. numbers 24, 33, 47, 50, 52, and 53.

extent of these eye movements as made possible in Experiment VII as compared with those in Experiment VI would clearly tend to decrease the effects of the retardation in the total critical reaction time of the subjects to a greater average extent in Experiment VII than in Experiment VI. In agreement with this we find that the average critical reaction time in Experiment VII approached more nearly to the average of the ordinary reaction times than was the case with Experiment VI.

We proceed to apply our view to the case of Experiment V, where there was, of course, no change from one positive to another positive peripheral stimulation. The cause of the cessation of sound drawing attention cannot, in such case, consist of the accumulation of the energy of the inhibited incoming excitation. We hold, however, that a logical extension of our view will also cover this case. Absence of sound is not, pyschologically, the absence of any auditory content at all; paradoxical as the statement may, at first sight, appear, the content 'absence of sound' may even attain to a considerable degree of intensity. For this content there must be a corresponding process of, at least, central excitation. Should there now be, as in the case of Experiment V, a more or less definite expectation for the continuance of a positive auditory sensation, and should this sensation suddenly cease, we should again arrive at the situation in which there are two processes of excitation which are in conflict with one another. There would then take place a similar, more or less momentary, retardation of the excitation corresponding to the content 'absence of sound,' as takes place when the excitation is actually an incoming peripheral one; as a consequence this content attains to a higher degree of attention as soon as this retardation is overcome.[45]

[45]Under such, and similar, circumstances "hört man die Stille." We have the same phenomenon in every day life if we are straining to hear any sound at all, even when there has been no immediately preceding period in which sounds were clearly audible.

It is also what should be expected on the view advanced in the text, that, if no attention has been given a sound or other stimulus, or if there has not been an expectation or a corresponding psychophysical disposition for such expectation, then the cessation does not tend to draw our attention. The special retarding effect referred to in the text would then not take place, or would be correspondingly slight and so, also, the resulting accumulation of energy. This does not imply, of course, that attention may not be given the sound for other reasons.

We come, in this way, to a conclusion which is in agreement with a view advanced by Titchener, who, after giving examples (partly from Fechner) which are intended to illustrate the attraction of attention to the cessation of unnoticed stimuli, goes on to state:—"Notice, first, that they are not simply instances of the cessation of unnoticed stimuli. In every case, foregone attention, and prolonged or frequent attention, is presupposed . . . On the other hand, I doubt if any really unnoticed stimulus attracts attention by its cessation. . . It is not that the absence

In the case of Experiment II the subject should, according to the conditions of the Experiment, have developed a more or less definite expectation, at the sixth repetition, for syllables which had already been shown, although not possibly for any particular syllable. The excitation coming from the new syllable would be incongruous with such expectation. We would, thus, again have the result that excitations which are incongruous with one another come into conflict. In so far the conditions, demanded by the theory for the positive attentional effect of the new, would again have been realised, and

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of stimulus commands attention, but that the expectant attention, a psychophysical predisposition, is disappointed, baffled by the silence. . . . the clock and street noises may be considered as . . . stimulating distraction. . . Their removal would . . . . make itself felt as a general restlessness or unsteadiness . . . we work from it to the cessation of the familiar stimulus; though I myself do not find it as frequently as I find a baffled expectation. A third and very similar explanation derives from Lehmann's law of the 'indispensableness of the habitual.' According to this law . . . the removal of an accustomed stimulus leaves a need, a *Bedürfness*. This comes to consciousness, in organic terms, as uneasiness or discomfort; and again we have a positive starting point for attention." *Lectures*, etc., 200-203. Lehmann states his agreement with Titchener, and adds that "Nur die Angewohnten und unter bestimmten Verhältnissen erwarteten, aber sonst unbemerkten Reize fesseln die Aufmerksamkeit, wenn sie ausbleiben." A. Lehmann, *Grund. d. Psychophysiol.*, 1912, 504. We hold, on the other hand, that Fröbes is wrong in advancing a similar explanation, as that given by Titchener (for the case of the cessation of unnoticed stimuli), as valid for the case where a stimulus, which has had our attention, ceases; in the sense, namely, that the cessation is not then itself a real stimulus for attention. He states that "Ebenso (as a determinant of attention analogous to, e.g., novelty) kann das plötzliche Aufhören des Reizes wirken; bekannt ist die oft angeführte Tatsache, dass das Stehenbleiben der Mühle den Müller weckt, dass ein verschwundenes Bild an der Wand sofort auffällt. Hier ist nicht das Fehlen des Reizes der eigentlichen Reiz der Aufmerksamkeit, sondern ein veränderter innerer Zustand. Das Aufhören des gewohnten Geräusches weckt eine Erwartungsspannung; es stellt sich eine aelgemeine Unruhe ein, was sicher ein positiver Reiz ist. Der betreffende sagt sich, dass etwas nicht in der Ordnung ist, und sucht nach der Ursache. Aehnlich spannend, ja beunruhigend ist der Eindruck einer plötzlichen Pause in einer rauschenden Musik, der Eindruck des plötzlichen Innehaltens des Redners, des Lehrers im Vortrag." J. Fröbes, *Lehrbuch der Experimentellen Psychologie*, **2**, 1920, 85. It is true that attention is directed to the cessation of the stimulus also in such ways, as is also testified to by our protocols from Experiment V. The subjects there did not, however, only note the cessation in the form of looking for a reason for the cessation but first of all noticed the cessation itself.

We have to distinguish, with Henning, between the primary and the secondary determinants of attention with regard to the novel and unexpected. Compare notes 40 and 43 as well as the following:—"Die starken *Gefühle*, welche das Interessante und Bedeutsame, das Origenelle und Seltene, das Wunderbare und Seltsame, das Neue und Fremdartige, das Ungewohnte und Überraschende sowie die Abwechselung ins Spiel setzt, wirken vornehmlich auf die instinktive Aufmerksamkeit, aber auch auf die unwillkürliche und willkürliche. Dabei treten was man Aeusserungen der

the phase of retardation could be followed by an increased degree of attention for the new syllable.[46]

In Experiments III and IV the different units were *presented* simultaneously, and the experimental conditions were not so arranged as to cause the expectation, previous to exposition, of movement or otherwise among the group of letters shown. The positive attentional effect observed in these cases therefore falls, it would seem, rather under Selz's *Gesetz der Distinktionsbeachtung* than under his law of *Widerstreitsbeachtung*; though it must be added that the former law was not limited in its application to objects presented simultaneously. Although, now, the letters in the groups were presented simultaneously, this does not, of course, imply that they must have been attended to simultaneously. We may, indeed, take it as certain, that the different letters were attended to successively to a considerable extent. Should a number of letters which are homogeneous be perceived, first of all, under these circumstances, some degree of expectation might have been aroused that all the letters would have this homogeneous characteristic. It is, however, by no means necessary to make this assumption to ex-

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Neugierde und des Wissenstriebes nennt, neben den Lebhaften Gefühlen Vorgänge des Beachtens und der Kontrolle, des Sichbesinnens und Nachprüfens auf mit ihren Beachtungsmotiven, Aufgabe—Richtungs—und Zielvorstellungen, Identifikationsprozessen u. a., wonach das Bewusstsein sich ausgiebig mit dem neuen Material beschäftigt. Mit der Abstumpfung der Gefühle fällt auch die Beachtung und das Interesse ab, infolge ausbleibender Reproduktionen und einer zunehmender Leere des Bewusstseins macht sich schliesslich Langeweile breit. Die Wirkung des Neuen und Fremden wird jedoch nicht allein von Dispositionen und Gefühlen getragen, sondern die Komplexqualität des Neuen und Fremden eröffnet reiche Möglichkeiten für die *Reproduktion* von Phantasiebildern und Stimmungen, insofern das Neue nicht in den Fesseln alter Assoziationen einbezogen ist. Die fortgesetzte Reproduktion neuer Phantasievorstellungen mitsamt ihren Stimmungen wirkt der Abstumpfung von Aufmerksamkeit und Gefühl entgegen." H. Henning, *Die Aufmerksamkeit,* 119. In view of these secondary effects W. B. Pillsbury's statement is certainly wrong that "The new thing will draw the attention but not hold it." He adds that "when the attention is held by the new thing, it is frequently because the new is not entirely new, and the familiar serves with the new to attract attention," the principle being that "ideas which had previously been in consciousness facilitated the entrance of the corresponding sensation when presented again." *Attention,* 1908, 50. This, however, could only bring about that attention is given to the familiar portion of what is 'new,' whereas the problem is really that attention is given to the new portion of what is 'new.'

[46]Should the subject, at presentation of the new syllable, have been expecting a syllable characterised by the *quality* of familiarity, any *quality* of unfamiliarity attaching to the new syllable would, again, be incongruous with his expectation; this should, on the theory, tend to increase the degree of attention accruing to the latter quality. We would then again have the phenomenon, as in Experiment V, where neither the inhibiting nor the positive attentional effect is limited to incoming peripheral stimuli from the object presented itself.

plain the attentional effect that was observed for the heterogeneous unit. For a conflict of excitations to take place, it would be sufficient if the one excitation were habitual, oft repeated or recent, as the result of which there would be present a tendency for it to perseverate. Such perseveration would then come into conflict with an excitation incongruous to it. In the Experiments now under discussion the fact, that the mass of the letters were homogeneous with regard to moving or being stationary, means the presence of an accumulation of causes for bringing about a perseveration corresponding to the homogeneous characteristic; and this perseveration would tend to extend also to the perception of the heterogeneous unit, thus bringing about the required conflict of incongruous excitations. It should be remarked that this view does not require that the first, or even the first attentive, perception of the heterogeneous letter must take place *after* a number of the letters with the homogeneous characteristic had been perceived or attended to first of all. It would be sufficient if the subject returned to the exception after observing letters with the homogeneous characteristic.[47] We have to add, furthermore, that a perseverating tendency must also have been present in the other Experiments on which we are reporting here and in which the different contents were presented in succession, and that it would have coöperated with, and may, on occasion, even have taken the place of a conscious expectation.

In agreement with this view we are led to the conclusion that Selz's two laws are, at bottom, formulations for a positive attentional effect resulting from essentially the same causal conditions. If we understand his meaning correctly, such a relation between the two laws was already suggested in general terms by Selz himself.[48] As protocols 54 to 57 testify, there were indications also in Experiment VIII that the unexpected heterogeneity in the colour of the non-reaction letters on the critical card attracted special attention, the area covered by them doubtlessly forming a favouring circumstance. The question arises as to why this tendency did not lead to a greater mean variation of the critical reaction times from the ordinary mean reaction time than was actually found to be the case, since such distraction from the reaction letter should have tended to lengthen the reaction times on the critical cards. The answer to this is, firstly, that there were counterbalancing factors—already mentioned—present at the same time which favoured quick recognition and undisturbed reaction on the prescribed letter.[49]

[47]There is also the possibility of a 'spread' of the perseverating tendency from homogeneous to heterogeneous units if attended to or perceived simultaneously. Tachistoscopic experiments directed to the solution of this problem are now being carried out.

[48]Cp. especially, the beginning of the quotation from Selz in note 2.

[49]Cp. note 39.

In addition, however, the important fact must also be taken into account, that the subjects had, according to instructions, not to react on letters having another than the required shape. The consequence of such an undertaking on the part of the subjects would be a tendency to give no more attention to the non-reaction letters than would be needed to become aware that they did not have the shape of the prescribed reaction letter. As soon as this state of awareness was attained with regard to any of the non-reaction letters the situation would be realised on which the determining tendency would be set in action to react by moving on to fresh letters. This holds not only for the case of the ordinary cards, but also for the case of the critical card in Experiment VIII. In those cases belonging to this Experiment in which the positive attentional effect for the heterogeneously coloured letters on the critical card clearly did take place before attention was attracted by the prescribed reaction letter, the determining tendency to move on would tend to come into action as soon as the increased attention, being given the non-reaction letters, had reached the stage of awareness of the fact that they did not have the required shape, i. e., even before the positive attentional effect need have attained the stage of full development. The view here presented also agrees with the observation that the suddenness with which an unexpected change takes place is a condition favourable for the change attracting attention.[50] One factor that is advanced by Titchener as causal condition for this effect is held to consist of the fact that "Sudden stimuli impinge upon nervous elements that have hitherto been free from stimulation of their particular kind, i. e., upon nervous elements of a high degree of excitability . . ." Though this is undoubtedly true in the case of sense organs which show the phenomenon of sensory adaptation in a more or less marked degree, it cannot be said to be generally true of the excitability of central residuals. For these, on the contrary, as the phenomena of reproduction and "apperceptive shift" show, the perseveration of recent and frequent excitations is favourable for their later excitability; hence lack of such preceding excitation should, other things being equal, be relatively unfavourable for such later excitability. We hold that the explanation for the positive attentional effect of the suddenness of the change must be sought for, at least in the main, in another direction. We can, as is well known, also develop the habit of not attending to distracting stimuli. This inattention is at first voluntary but eventually comes to take place in a more or less involuntary way. Expressed in other words, this means that a determining tendency has been formed, and strengthened by repeti-

[50]Cp. Titchener, *Lectures*, etc., 192-3, and K. M. Dallenbach, *Amer. J. Psychol.*, **24**, 1913, 482-3, both of whom give further references on the phenomenon. See also note 39.

tion, to act in a certain attentional way when an anticipated situation (i. e., the 'distraction') occurs. So, e. g., this materially assists us successfully not to attend to distractors which remain at least comparatively uniform as objective stimuli.[51] Should, however, sudden and irregular variations of these stimuli take place, for which the more specifically directed determining tendency and habit not to attend cannot have been so well developed, then the distracting influence is greater. It is increased in the sense, firstly, that the inhibiting effect of such determining tendency is now absent or at least less strong. The sudden change is, however, the introduction of an unexpected heterogeneity. On our view, then, we would also in such cases of an increase of distraction have to do not merely with "dem unverminderten Reiz des Objektes" but also with an increased attentional effect accruing to it. Again, should the change be a gradual one, but without our having decided not to attend to it, it still happens that it draws attention less easily, other things being equal, than if it were introduced suddenly. According to the view advanced here the following must be one, at least, of the causes bringing about this effect. Should the change attain its maximum gradually, then the excitation also changes gradually. Consequently the maximum change in the excitation has been preceded not by an incongruous but by a very largely congruous excitation, even if no conscious state of expectation for similar future changes as those in the past has been aroused. The conditions needed for a conflict between the perseveration of the previous excitations and the present excitation, as coming from the maximum, have, therefore, now largely disappeared. It is clear that the extent to which the latter is the case will depend upon how gradual the change was.

Summary

1. A number of Experiments are described in which attentional effects occur in agreement with Selz's laws of *Distinktionsbeachtung* and *Widerstreitsbeachtung*.

[51]We may, in addition, decrease a distracting effect by taking a more energetic positive decision to attend to the work from which we are being distracted. A decision of this nature is, of course, not the same thing as conscious content, as the decision not to attend to the distraction. We may, furthermore, become practiced in the quick alternation of attention in different directions, as when we are given two kinds of work to do "simultaneously," so that the one kind of work does not, eventually, prevent us so much from attending to the other as at the beginning. Compare, e. g., A. J. Hamlin, Attention and Distraction, *Amer. J. Psychol.*, 8, 1896, 49. E. B. Titchener, *Lectures*, etc., 216-7; E. Jacobsen, Further experiments on the inhibitions of sensations. *Amer. J. Psychol.*, 23, 1912, 348; R. Vogt, Über Ablenkbarkeit und Gewöhnungsfähigkeit, *Kraepelin's Psych. Arbeiten*, 3, 1899, 62.

2. It is shown that these attentional effects are sometimes reversed under the conditions stated in the laws, and that this phenomenon as well as that of apperceptive shift forms real exceptions to the laws as formulated by Selz.

3. A theory is developed as to the differences among the causal conditions which bring about these differences in attentional effect; the theory resting, in part, on a hypothesis stated by Henning with regard to an accumulation of energy, in a limited area, consequent on the temporary inhibition of one excitation by another not congruent with it.

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"HEARING" WITH THE SENSE OF TOUCH*

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I. INTRODUCTORY

The recent efforts of Professor Robert H. Gault[1] and others[2] to teach the deaf to "hear" with the sense of touch led the writer to undertake the present investigation. If the deaf, or others, can ever learn to interpret speech or music with the sense of touch, this sense must possess certain characteristics. It occurred to the writer therefore that it would be of some value, especially to those interested in the education of the deaf, to know just to what extent the sense of touch is endowed with the analytic functions required for hearing speech or music.

Accordingly, therefore, the investigation reported in this paper consists of (1) an experimental determination of the sensitivity and sensibility characteristics of the sense of touch to a vibrating body, and (2) a comparison of these characteristics with the analogous ones possessed by the sense of hearing. The individual with normal hearing possesses the following fundamental characteristics:

(1) He can hear sound vibrations throughout a range of approximately ten octaves, from 20 d. v. to 20,000 d. v., provided the intensity of the vibrations is within certain bounds of the minimal and maximal thresholds.[3]

(2) He can distinguish small differences of amplitude of sound vibrations, the smallest discernible difference amounting to approximately ten per cent for feeble sounds and diminishing to about five per cent for moderately loud and very loud sounds.[4]

(3) He can distinguish small differences of frequency; the smallest discernible difference amounting approximately to one per

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[1]See Professor Gault's papers on Tactual Interpretation of Speech, etc.: *J. Abn. Psychol. and Soc. Psychol.*, 1924, **19**, No. 2; *J. Wash. Acad. Sci.*, 1925, 15, No. 14; *Sci. Mo.*, 1926, **22**; *Arch. Otolar.*, 1926, **3**, 121-135; *J. Appl. Psychol.*, 1926, **10**, 75-87; *J. Abn. Psychol. and Soc. Psychol.*, 1926, **21**, 4-13.

[2]Goldstein, M. Practical demonstration of modern pedagogic methods in training the deaf child. *Laryngoscope*, 1926, **36**, 31-42.

[3]Fletcher, H. Physical Measurements of Audition, *J. Franklin Inst.*, Sept., 1923, 289-326; Wegel, *Proc. Nat. Acad. Sci.*, July 15, 1922.

[4]Knudsen, V. O. Sensibility of the ear to small differences of intensity and frequency, *Phys. Rev.*, 1923, **21**, 84-102.

cent for very low and very high pitched tones, and approximately three-tenths of one per cent for medial pitched tones.[5]

(4) He can make other auditory discriminations, such as quality, consonance, dissonance, beats and rhythm, but these are relatively of minor importance compared with the prime requirements of normal sensitivity to the entire frequency range used in speech and music, and of the normal sensibility to small differences of loudness and pitch.

It would seem therefore that the experimental work in this investigation should include the following:

(1) A determination of the lower and upper frequency limits of a vibrating body to which the sense of touch responds.

(2) The sensitivity of the sense of touch to a vibrating body as a function of the frequency of vibration.

(3) The sensibility of the sense of touch to small differences of amplitude of a vibrating body.

(4) The sensibility of the sense of touch to small differences of frequency of a vibrating body.

II. History

A search of the literature indicates that very little work has been done in studying the sensation of feeling vibrations. M. von Frey,[6] in 1915, reported his studies on the sensation of feeling of vibration. He used tuning forks of from 60 to 100 double vibrations per second. His results, he claims, seem to justify the conclusion that the pressure receptors in the skin mediate the feeling which results from touching a vibrating body. He observed that placing the tuning fork in contact with bone intensified the sensation. He felt that this was attributable to the spread of the stimuli to a greater skin area, thus activating a greater number of pressure receptors.

For the past twelve years David Katz[7] has made important contributions to experimental studies of touch, including the feeling of vibration. He regards the sense of vibration as an independent aspect of touch, thus disagreeing with von Frey, who believes that the feeling of vibration is mediated by the pressure receptors. Katz supports his claim that the vibration sense is an independent aspect of touch with the following facts:

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[5]Knudsen, V. O., *loc. cit.*; Young, R. C., *Amer. J. Psychol.*, 1926, **37**, 313-329.

[6]von Frey, M. Physiologische Versuche über das Vibrations-gefühl, *Zsch. f. Biol.*, 1915, **65**, 203.

[7]Der Aufbau der Tastwelt, *Zsch. f. Psychol.*, (Ergänzungsband, No. 11), Leipzig, 1925. (A review of Katz's book, by M. J. Zigler, will be found in *Psychol. Bull.*, 1926, **23**, No. 6.)

(1) If the elbow be pressed against a sounding box which supports a large vibrating tuning fork, a sensation of vibration will be felt in the hand even though it is not in contact with the vibrating body.

(2) The tip of the tongue is very sensitive to pressure but responds very poorly to vibration.

(3) There are sufficient nerve receptors in the skin to supply the carriers for this and also the other cutaneous qualities. Thus, the cutaneous senses do not conflict with the law of specific nerve energies.

Robert H. Gault,[8] during the past two years, has demonstrated that the sense of touch can be trained to identify many speech sounds with an encouraging degree of accuracy, provided the sound vibrations are communicated to the tips of the fingers or the palm of the hand by means of the diaphragm of a special telephone receiver. The individual under test feels the "patterns" of the different speech sounds, and with adequate training and experience remembers the "feel" of these different "patterns." It will be shown later in this paper that within certain limits the sense of touch is as well adapted as the cochlea to respond to the intensity "patterns" or "envelopes" of different speech sounds.

Max Goldstein and his assistants at the Central Institute for the Deaf[9] in St. Louis have found that many of their deaf pupils respond to speech sounds which they feel by placing their fingers in contact with a paper stretched over the large end of a megaphone.

The meager quantitative data available concerning the sense of touch to vibrating bodies, together with the recent efforts to utilize this sense in teaching the deaf to interpret speech tactually, would seem to warrant a thorough investigation of this sense. The work which follows in this report is only of a preliminary nature. The results, however, are sufficiently complete and accurate to determine the general characteristics of the sense of touch to sound vibrations, and to indicate what measure of success should be anticipated from utilizing this sense as a substitute for hearing among the deaf.

III. Apparatus

The apparatus required for testing the sense of touch to sound vibrations should possess the following features:

(1) The apparatus should produce in a diaphragm, or in a reed, or in some such body, simple harmonic vibrations throughout the entire frequency range of feeling. It would seem advisable to

[8] *Loc. cit.*
[9] *Loc. cit.*

devise a vibrator which would be well adapted to mediate the vibrations to the tips of the fingers.

(2) The apparatus should be equipped to facilitate a change of either the frequency or the amplitude of vibration by any desired amount.

(3) The apparatus should be equipped with suitable means for determining the frequency or amplitude, or any given changes of frequency or amplitude, of the vibrating body.

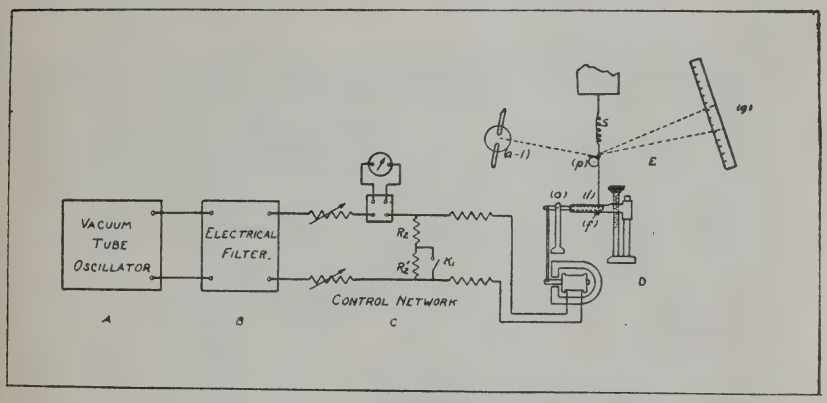

FIGURE 1

GENERAL ARRANGEMENT OF APPARATUS FOR DETERMINING THE SENSITIVITY AND SENSIBILITY CHARACTERISTICS FOR THE FEELING OF VIBRATION.

The apparatus shown diagrammatically in Fig. 1 possesses most of these required features, and is the principal part of the equipment used in this investigation. It consists of (A) a vacuum tube oscillator, which will generate frequencies from 10 to 50,000 vibrations per second; (B) an electrical filter which renders the vibrations simple harmonic; (C) an electric network including a thermocouple and galvanometer for controlling and measuring the current input into the vibrator; (D) an electromagnetic telephone receiver adapted to communicate its vibrations to a pivoted lever; (E) an optical system for determining the amplitude of vibration of the vibrating lever.

The oscillator will generate an alternating current constant in value, of any desired frequency. The instrument is calibrated so that any frequency, above 10 d. v.,[10] can be obtained by an appropriate setting of the dials on the oscillator. The alternating current output from the oscillator is approximately 10 watts. For frequencies

[10]Hereafter frequency of vibration will be indicated by d.v. after the number; thus, 256 d.v. means 256 double vibrations per second.

below 512 d. v. the energy is sufficient to operate the vibrator designed for this investigation with an amplitude approaching its upper safe limit. The alternating current output from the oscillator is not a pure sine wave, but by means of a suitable low-pass filter (B) all undesirable harmonics are eliminated. The resulting wave form of the alternating current, after it has passed through the filter, is approximately simple harmonic, as is shown in Fig. 2, which is an oscillogram of the current for a frequency of 64 d. v. An inspection, with the oscillograph, of the other frequencies used in the tests indicated similarly that all of them closely approximated simple harmonic motions.

The thermocouple and galvanometer were calibrated so that readings of the deflection of the galvanometer and the dial box resistances in the network (C), including the impedance of the receiver, make it possible to determine the current actuating the vibrator. This current can be varied from zero to a maximal value, principally by the shunt resistance R_2. When testing for the smallest noticeable difference of amplitude of vibration the resistance R'_2 is periodically added to R_2 by means of key K_1.

The vibrator (D) is shown in more detail in Fig. 3. It is made up principally of the working mechanism of a Western Electric Cone Type Loud Speaker. The armature of the loud speaker is attached to the short end of an aluminum lever (l) which is tightly and carefully pivoted at (o). Several types and sizes of levers were tried out and the most sensitive one was selected for the actual tests. This lever is 11.70 cm. long, .09 cm. wide, and .48 cm. thick. The short end of the lever is only .50 cm. long, so that the motion of the armature is increased 21-fold at the distal end of the lever, the end which makes contact with the finger tips in the actual tests. A horizontal U-frame (f), mounted on a vertical micrometer, fits over the vibrating lever and serves as a support for the finger tips. By proper adjustment of the micrometer the finger tip can be made to just barely touch the vibrating lever. Preliminary tests indicated that this slight pressure of the finger tip against the vibrating lever did not alter the amplitude of vibration of the lever, especially at small amplitudes in the proximity of the threshold of sensation.

The optical system (E), used for magnifying the amplitude of vibration of the aluminum lever, is similar to the optical system of Professor D. C. Miller's Phonodeik. A very fine silk fiber is attached to the distal end of the lever; from the lever it extends to, and passes once around, the small pivoted pulley at (p), which is part of a watch movement; and from the pulley it extends to the spring (s) which holds the fiber under tension. A tiny mirror mounted on the pulley (p) reflects a small spot of light from the

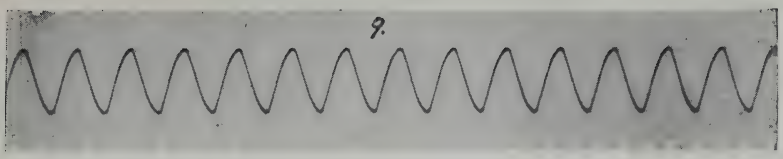

FIGURE 2

Oscillogram Showing the Wave-Form of the Alternating Current, Generated by the Vacuum Tube Oscillator, After It Has Passed Through the Filter.

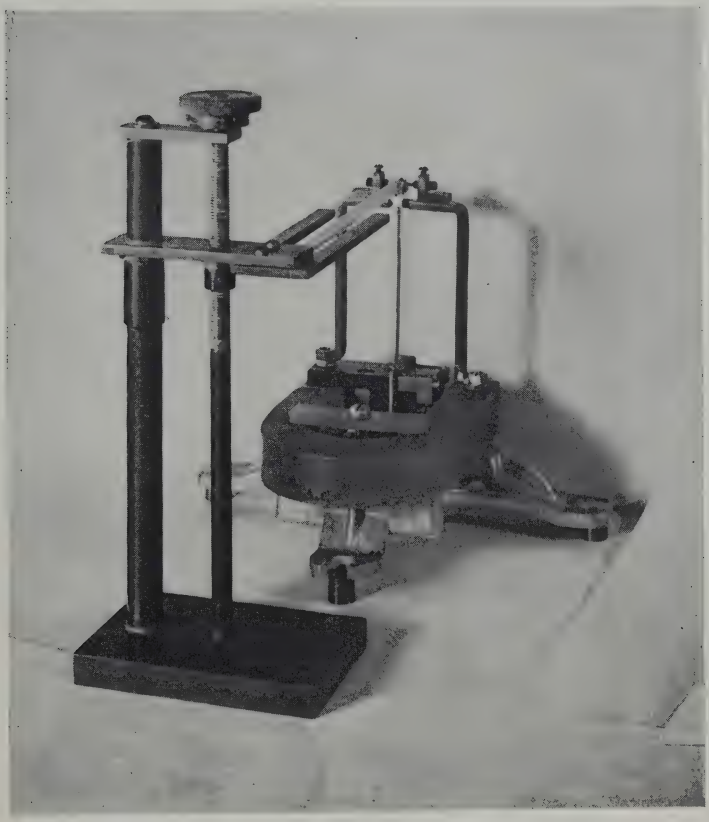

FIGURE 3

Photograph of the Vibrator, Showing the Mechanical Arrangement of the Armature, the Vibrating Lever and the Micrometer U-Frame for Bringing the Finger into Contact with the Vibrating Lever.

arc light (a. l.) to the graduated screen (g). When the lever is at rest there is a single small spot of light on the screen. When the lever is vibrating this spot spreads out into a narrow ribbon the length of which is proportional to the amplitude of vibration. In this way the motion of the distal end of the vibrating lever is magnified 1950-fold. The natural period of the vibrating portion of the optical system was considerably shorter than any used in the tests, and hence no corrections were necessary to determine the amplitude of motion of the lever in terms of the spread of the spot of light on the screen.

The magnification factor of the optical system was determined by direct calibrations, all made at the frequencies used in the tests, namely 32 d.v., 64 d.v., 128 d.v., 256 d.v., 512 d.v., 1024 d.v., and 1600 d.v. The calibrations were made by means of a traveling microscope which measured directly the amplitude of vibration of the lever. Platinum points were provided on the end of the vibrating lever and also on a metal arm which was attached to the traveling microscope. A local circuit, consisting of a high resistance, a pair of low-impedance head phones and a battery, was connected across the platinum points. The vibrating lever was then set in motion with a certain amplitude and the traveling microscope adjusted until the platinum points just barely touched, periodically, at the maximal displacements of the vibrating lever. This adjustment could be made with satisfactory accuracy by listening with the head phones. In this way the motion of the vibrator was calibrated in terms of the deflection of the spot of light, and also in terms of the current actuating the vibrator. The calibrations indicated that the amplitude of the vibrator was approximately proportional to the current actuating the vibrator. These calibration curves simplify very much the process of measuring the amplitude of vibration. It is only necessary to measure the current flowing through the vibrator and read off the calibration curve, for the corresponding frequency, the amplitude of vibration. The magnification factor of the optical system was further checked by measuring the displacement of the spot of light on the screen for a known displacement of the vibrating lever.

The apparatus included certain other special devices, but these will be described in connection with their specific uses.

IV. Experimental Results

(a) *The lower and upper frequency limits to which the sense of touch responds.* Preliminary tests on the sensitivity of the different fingers and the palms of the hands indicated that the end of the right index finger was probably the most suitable receptor for feeling

vibration. Unless specifically stated, the reader will understand that the tests were made with this finger touching the vibrating lever.

When the frequency of vibration was less than about 12 to 15 d.v., the successive impacts of the vibrating lever were separately sensed. They were sensed as a series of gentle impacts against the finger tip. When the frequency of vibration was greater than about 15 to 18 d.v., the successive impacts produced a constant "tingling" sensation. Tests of the lower frequency limit were conducted with two individuals, the writer and Mr. Lewis Delsasso, who assisted the writer very ably throughout all of the experiments. The lower frequency limit seemed to depend to a certain extent upon the amplitude of vibration, greater amplitudes producing a slight elevation of the lower frequency limit. The average results of a number of measurements of the lower frequency limit for the two observers are given in Table I.

TABLE I

LOWER FREQUENCY LIMITS

Observer	*Frequency*
L. D.	12.5 to 15.5 d.v.
V. O. K.	15.2 to 18.0 d.v.
Average	15.3 d.v.

No conclusive data were obtained for the upper frequency limit. This limit is certainly a function of amplitude, as would be expected, and a more powerful vibrator than the one used in these tests will be required to ascertain the upper frequency limit. With the amplitude of vibration attainable in these tests the upper frequency limit was approximately 1600 d.v.[11] The data obtained would indicate that the upper frequency limit is certainly higher than this, possibly as high as 3000 to 4000 d.v., or even higher with a sufficiently great amplitude of vibration. The reason for this anticipation will be apparent from a consideration of the sensitivity-frequency curve, Fig. 4, which will be discussed in the following section.

(b) *The sensitivity of the sense of touch to a vibrating body, as a function of frequency, between 32 d.v. and 1600 d.v.* The sensitivity of feeling of vibration was measured, following the usual method, by determining the amplitude of vibration required to elicit a just barely discernible sensation of feeling of vibration. This minimal threshold was obtained by approaching the threshold from both sides; first starting with an unmistakable stimulus and reducing the amplitude to the point of uncertainty, and second, starting with an amplitude well below the minimal threshold and increasing

[11]Landlois, Lehrbuch d. Physiol. d. Menschen, 1880, states that vibrations of strings are recognizable as such by the finger, even at a frequency of 1552 d. v.

the amplitude until the feeling of vibration is just barely discernible. The average of two pairs of such readings is probably accurate to about ten per cent. This is the probable accuracy of the results obtained in this part of the investigation. It was found that with practice and care the accuracy was of the same order as is attained in determining the minimal threshold for hearing. The variation from day to day amounted to about fifty per cent.

The amplitude of vibration, for a given frequency, seemed to be the important factor in determining the intensity of the feeling of vibration,[12] and for that reason the amplitude of vibration will be used for measuring the sensitivity. This is consistent with what is known about the sense of touch to pressure, where deformation of the skin seems to be the determinative factor.[13] The sensitivity, measured by the amplitude of the vibrating body at the minimal threshold, is, of course, measured inversely; that is, the smaller the amplitude at the minimal threshold the greater the sensitivity.

When, for a given trial, the minimal threshold has been located by adjusting the resistances in network (C), Fig. 1, the readings of the

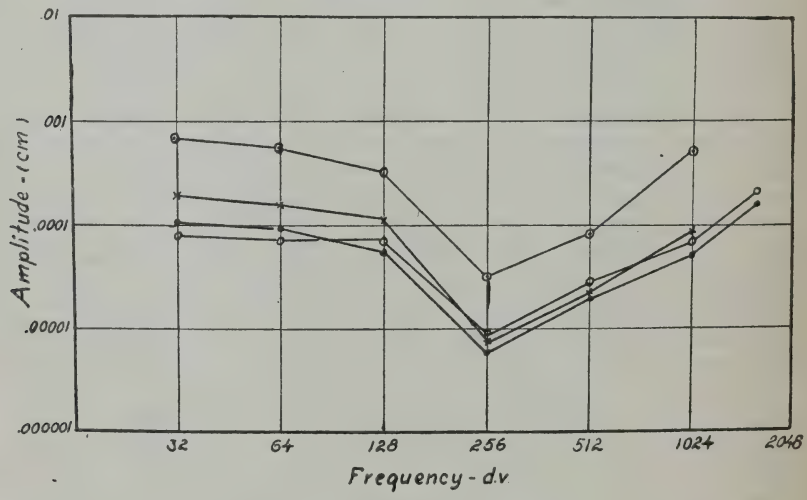

FIGURE 4

THE FREQUENCY-SENSITIVITY CURVES FOR FOUR INDIVIDUALS. The frequency of vibration is indicated along the horizontal axis and the amplitude of vibration which can be just barely discerned is indicated along the vertical axis. Note that on the average the smallest discernible amplitude of vibration is approximately .0001 cm.

[12]This conclusion was reached after testing several vibrating levers of different lengths and widths.

[13]See Schäfer's Textbook of Physiology, 1900, Vol. II, p. 924.

resistances and the galvanometer deflection are recorded. From these readings the current actuating the vibrator is easily calculated, and then, using the appropriate calibration curve, the amplitude of vibration of the lever at the minimal threshold is read off directly.

The results obtained from tests of four individuals are shown in Figure 4. Here the sensitivity, expressed in terms of the amplitude of vibration required to elicit a barely discernible sensation, is plotted vertically on a logarithmic scale as a function of the frequency of vibration, which is plotted horizontally, also on a logarithmic scale. No measurements were obtained above 1600 d.v.

A number of features concerning the sensitivity curves in Fig. 4 are of interest. (1) The magnitudes of the amplitudes at the minimal thresholds are exceedingly small. They vary from slightly less than .00001 cm. to nearly .001 cm., that is, from about one-tenth the wave length of red light to about ten times this wave length. (2) The maximal sensitivity occurs for a frequency of about 256 d.v. The sensitivity is nearly uniform for frequencies below 128 d.v. and decreases quite rapidly for frequencies above 256 d.v. (3) Three of the four observers show approximately the same sensitivity, especially for the higher frequencies, whereas one of the four shows markedly diminished sensitivity throughout the entire frequency range. This one observer had sustained a radium burn on the right hand which had probably diminished his sensitivity. In fact, the tip of his left index finger was more sensitive than the corresponding right one, but even his left index finger was considerably less sensitive than the right or left fingers of the other three observers. (4) Since the sensitivity is decreasing quite rapidly at the higher frequencies, it is probable that the upper frequency limit which can be felt as a vibration is not much more than one or two octaves above 1600 d.v., the highest frequency which could be felt with the apparatus used in this investigation. An extrapolation of the sensitivity curves to higher frequencies, however, would seem to justify the conclusion that vibrations as frequent as 3000 to 4000 d.v. should be felt. A study of this upper frequency limit may yield additional information concerning the mechanics of feeling vibrations and also concerning the general nature of fusion of separate sensations.

As the amplitude of vibration is increased above the minimal threshold, the intensity of the feeling of vibration increases until it becomes unpleasant or even painful. The apparatus used for the sensitivity measurements was not powerful enough to approach this limit, and therefore special apparatus was devised to investigate the maximal or painful threshold. The apparatus consisted of a heavy brass lever which was set into forced harmonic vibration by having one end of the lever follow a sine wave pattern which was cut into

the rim of a brass disc. By rotating the brass disc the lever was set into simple harmonic motion. The amplitude is determined by the depth of the sine wave groove in the brass disc and the distance from the axis of the lever to the position of the finger on the lever. The device was serviceable for producing intense vibrations up to about 100 d.v. At higher frequencies it became unstable and unreliable. At a frequency of 64 d.v. the vibration became painful at an amplitude of .235 cm. At 32 d.v. the painful amplitude could not be reached with the apparatus but it was certainly more than .40 cm. These intense vibrations produced extreme fatigue or "after pain" in the finger. The finger felt numb for several minutes after the tests. The painful amplitude at 64 d.v. is approximately 2500 times the minimal threshold amplitude. For lower frequencies the painful limit is even further above the minimal threshold, and for higher frequencies the painful threshold is nearer the minimal threshold. This would be expected since the energy of vibration increases with the square of the frequency.

The extremely small amplitude of vibration at the minimal threshold is capable of explaining why even very smooth surfaces produce sensations of vibration when the degree of roughness of such surfaces is perceived by moving the fingers over the surface. Katz is unable to decide whether the sensation of vibration is completely absent when the finger is moved over a "perfectly smooth" surface.[14] Surfaces as smooth as window glass have surface irregularities (departures from a plane surface) as large as the wave length of visible light, and, according to the data on the amplitude of vibration at the minimal threshold, these irregularities are large enough just barely to elicit the sensation of vibration when the finger is moved over the surface. Optically plane glass, on the other hand, has surface irregularities of the order of only one-twentieth of a wave length, or less. These, according to the sensitivity data, are not sufficient to produce a sensation of vibration. The writer finds that when his finger is moved over window glass a sensation of vibration is just barely elicited; when it is moved over plate glass the sensation of vibration is doubtful; and when it is moved over optically plane glass the sensation of vibration is completely absent. According to the observed data on sensitivity to vibration, any surface should elicit a feeling of vibration when the finger moves over it, provided the surface irregularities (departures from a plane surface) exceed twice the minimal threshold amplitude of vibration; that is, provided the surface irregularities are greater than about .00002 cm. This is roughly confirmed by the simple tests of moving the finger over the window, plate, and optically plane glass surfaces. In these tests the finger should be

[14]Katz, *loc. cit.*

moved with such a speed that it will move over the "peaks and hollows" of the surface at a rate of say 200 to 500 "peaks" a second, the frequency range for which the feeling of vibration is most sensitive.

(c) *The sensibility of the sense of touch to small differences of amplitude of a vibrating body.* The method employed to determine the sensibility of the sense of touch to differences of amplitude of a vibrating body was essentially that used by the writer for determining the sensibility of the ear to small differences of loudness.[15] The sensibility is expressed in terms of the ratio of the smallest discernible increment or decrement of amplitude of vibration to the entire amplitude of vibration. This will be denoted by $\Delta A/A$. It is an inverse measure, that is, the smaller $\Delta A/A$ the greater the sensibility. Custom has given a preference to this measure of sensibility as applied to other senses, as vision and audition, and therefore this measure will be employed in describing the results of this part of the investigation. The observer adjusts his finger tip, by means of the micrometer U-frame, to the free end of the vibrating lever which has been set in vibration at a determined amplitude. The amplitude is then increased and decreased periodically by opening and closing the key K_1 (see Fig. 1). When K_1 is open the resistance R'_2 is added to R_2, thus increasing the shunt resistance across the vibrator which produces a corresponding increase in the current actuating the vibrator. When K_1 is closed the resistance R'_2 is effectively reduced to zero, thus decreasing the shunt resistance across the vibrator which produces a corresponding decrease in the current actuating the vibrator. If R'_2 be large enough the observer will feel a periodic increase and decrease in the intensity of the feeling of vibration as the key K_1 is opened and closed. By reducing R'_2, as K_1 is periodically opened and closed about once a second, the variation in the intensity of feeling of vibration will become less and less until finally the vibration is sensed as a steady one, free from any periodic variations of intensity. Again, starting with R'_2 very small and gradually increasing it, as K_1 is periodically opened and closed, there will occur a time when the flutter in the intensity of the feeling of vibration becomes just barely discernible. The points of appearance and disappearance of this flutter are approximately the same. By noting the average value of R'_2 for a number of such appearances or disappearances of the flutter, it is easy to calculate the smallest perceptible change of amplitude of the vibrator.[16]

[15] *Loc. cit.*

[16] The method of calculating $\Delta A/A$ is essentially that given in the writer's paper on the Sensibility of the Ear to Small Differences of Intensity and Frequency, *loc. cit.*

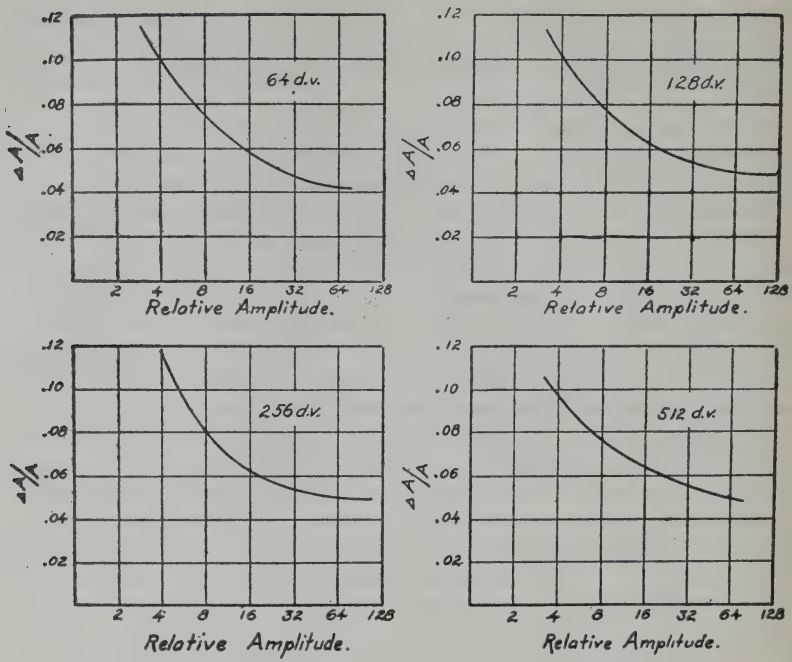

FIGURE 5

CURVES SHOWING THE INTENSITY-SENSIBILITY FOR THE FEELING OF VIBRATION AS A FUNCTION OF THE AMPLITUDE OF VIBRATION. The curves are for frequencies of 64 d.v., 128 d.v., 256 d.v., and 512 d.v. Note that the sensibility is approximately the same for all frequencies and that the smallest discernible change of amplitude decreases as the amplitude increases, approaching a constant value of about five per cent.

The average results of the measurements on the sensibility to differences of amplitude of vibration, for two observers, are shown in Fig. 5. The ratio of the smallest discernible change of amplitude to the total amplitude, $\Delta A/A$, is plotted as a function of the amplitude of vibration. The graphs in Fig. 5 are for frequencies of 64 d.v., 128 d.v., 256 d.v., and 512 d.v. In each graph the amplitude of vibration corresponding to the minimal threshold is at the origin of the amplitude axis. The amplitude, expressed in terms of the minimal-threshold-amplitude, is then plotted upon a logarithmic scale, each unit on this axis corresponding to a doubling of the amplitude. The intensity sensibility, expressed inversely by $\Delta A/A$, is indicated along the vertical axis. The intensity sensibility was determined for several amplitudes of vibration, ranging between four and about 100 times the minimal-threshold-amplitude. Each point on the curves is

the average of at least four separate judgments by each observer. These separate judgments vary among themselves as much as ten to fifteen per cent. Here, as in other instances of determining the smallest discernible differences, the observer is conscious that his judgment of the difference, near the threshold difference, changes gradually and continuously from complete certainty of a difference to complete uncertainty of a difference. In a comprehensive study of the intensity sensibility more data should be obtained than are here presented, but for the immediate purpose the results indicated by the sensibility curves in Fig. 5 are wholly reliable and adequate.

The graphs in Fig. 5 show that $\Delta A/A$ decreases as the amplitude increases. $\Delta A/A$ is approximately the same function of intensity for all frequencies of vibration and tends to approach a constant value of .05 at amplitudes of about 50 to 100 times the minimal-threshold-amplitude. The graphs are remarkably similar to those obtained by the writer for hearing.[17] The $\Delta A/A$ for hearing approaches approximately .05 for moderately loud and very loud sound, and increases as the loudness is decreased. This is shown in Fig. 6 which is a typical curve of intensity sensibility, for hearing. It appears, therefore, that the sensibility of the sense of touch to

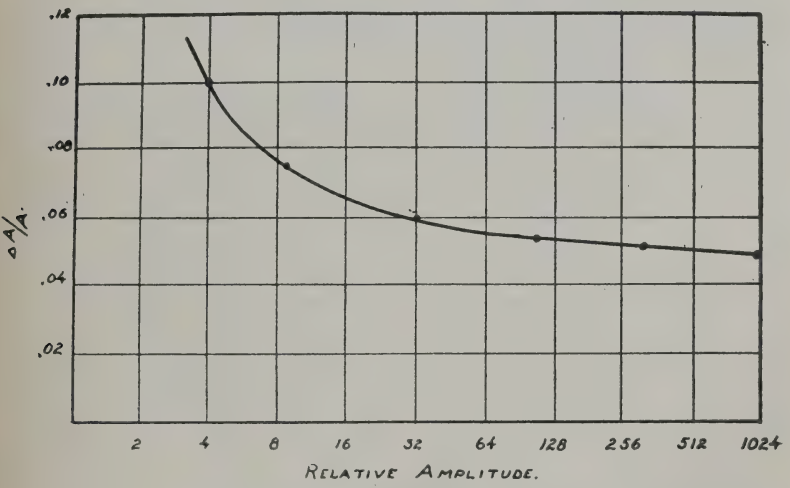

FIGURE 6

CURVE SHOWING THE INTENSITY-SENSIBILITY FOR HEARING. Note the close resemblance of this curve and those in Fig. 5. Hearing and feeling of vibration are remarkably alike in this particular function.

[17] *Loc. cit.*

small differences of intensity of a vibrating body is, qualitatively and quantitatively, like the sensibility of the cochlea to small differences of intensity. It is qualitatively, but not quantitatively, like the sensibility of the eye to small differences of light intensity.[18]

The same modification of the Weber-Fechner law proposed by Nutting[18] for vision, and shown by the writer to apply to hearing, holds also for the sense of feeling of vibration. The form of the equation expressing $\Delta A/A$ as a function of the amplitude of vibration is

$$\Delta A/A = F + (1—F\ (A_o/A)^n \qquad (1)$$

F = the constant minimal value the ratio $\Delta A/A$ approaches at large amplitudes. F = approximately .05 for the sense of feeling of vibration.

A_o = the amplitude of vibration at the minimal threshold.

n is an arbitrary number depending upon the frequency of vibration.

This expression holds quite satisfactorily for both small and great amplitudes of vibration. At the minimal threshold, $A_o/A = 1$ and therefore $\Delta A/A = 1$, as is required at the minimal threshold, since the increment ΔA is equal to the total stimulus A. Also, at great amplitudes, A_o/A becomes small and therefore $\Delta A/A = F$; that is, the Fechner ratio becomes a constant as is borne out by experiment.[19]

(d) *Sensibility of the sense of touch to small differences of frequency of a vibrating body.* In these experiments two independent sources of frequency were employed. This was found to be necessary because a just noticeable change of frequency proved to be so large that it was impossible to make these changes on a single oscillator without an accompanying change of amplitude. By using two oscillators, it was possible to maintain at the same amplitude the two vibrations which were to be compared. It was also necessary in these tests to make use of the method of right and wrong guesses, since the apparatus, including two oscillators, did not lend itself to the "flutter" method. Before each test the observer felt the two vibrations which were to be compared, in order that he might recognize the distinguishing characteristics of different rates of vibration.

[18]Nutting, P. C. The luminous equivalent of radiation, Bureau of Standards, 5, 261.

[19]Fullerton and Cattell, Pub. of Univ. of Penn., Phil. Series, No. 2, May 1892, account for the departure from Weber's law on the basis of the error of observation increasing as the square root of the magnitude of the stimulus. The expression (1) would, of course, include this and other possible factors. It would be of interest to determine whether (1), which holds for seeing, hearing, and feeling of vibration, expresses a general law of intensity-discrimination for other sensations.

After a little experience it was fairly easy to distinguish a difference of frequency provided the difference was large enough.

The results of these tests, for two observers, for test frequencies of 64 d.v., 128 d.v., 256 d.v., and 512 d.v., are given in Table II. The frequency sensibility is expressed inversely by $\Delta N/N$, that is, the ratio of the smallest discernible difference of frequency ΔN to the frequency of vibration N. The amplitude of all test frequencies was adjusted to 50 times the minimal threshold amplitude. This insured equality of intensity, on a sensation scale, of the two vibrations which were to be compared with regard to frequency of vibration. It also insured uniformity of intensity of all vibrations used for these tests.

TABLE II

FREQUENCY SENSIBILITY OF THE SENSE OF TOUCH

N	$\Delta N/N$ (L.D.)	$\Delta N/N$ (V. O. K.)
64 d.v.	.12	.15
128 d.v.	.22	.31
256 d.v.	.08	.10
512 d.v.	.20	.35

The results in Table II are intended to show only the order of magnitude of the frequency sensibility of the sense of touch to a vibrating body. Many more tests with more observers would be required to determine precisely the average sensibility for normals, but the preliminary results obtained are approximate and indicate that ordinarily the frequency of a vibrating body must change as much as 15% to 30% before a difference is noticeable. The ear, on the other hand, can appreciate a change of frequency of one per cent or less. It appears, therefore, that the sense of touch is not well adapted to recognize small changes of frequency. Probably as many as 25 or 30 gradations of frequency can be distinguished between 16 d.v. and 1600 d.v., whereas the ear can sense approximately 1000 gradations of pitch between these two frequency limits.

Probably everyone has noticed the difference in the feeling of low and high frequency vibrations. For example, that low and high frequency vibrations produce their own characteristic and different sensations when sensed by the tactile nerves is borne out by the familiar response of deaf children to low and high pitched tones. A deaf child standing on a piano can appreciate readily the characteristic "feel" of low or high tones sounded on the piano.

The average normal person has almost no occasion for determining frequency of vibration with the sense of touch. It is possible that training might improve this frequency sensibility, but the development of this sense to a high degree seems very improbable as the tactile nerves have no special end organs like the cochlea, whose mechanism is peculiarly well adapted for resolving small differences

of frequency. In this connection, it would be of interest to determine the frequency sensibility for a number of deaf individuals, as Helen Keller, Willeta Huggins, and Eugene Sutermeister, who have used their tactual sense for the interpretation of speech and music. Sutermeister, for example, after fifty-two years of deafness, suddenly found that he could enjoy music.[20] He senses intensity and rhythm, and can distinguish simple from complex music, but it does not seem likely to the writer that he or anyone can distinguish small differences of frequency, differences as small, for example, as a semi-tone interval.

(e) *Probable bearing of the experimental results on the nature of the feeling of vibration.* The findings in this investigation would seem to support the conclusions of Weber and Katz. The former described the feeling of vibration as a sense intermediate between touch and hearing. Katz points out that genetically the development of the sense of feeling of vibration is the first step in the evolution of an organism for the perception of distant disturbances; "it is the genetic antecedent of the sense of hearing."[21] The feeling of vibration is distinctly different from the feeling of pressure, or any of the other cutaneous sensations, and if we accept a neurone theory, or a law of specific nerve energies, it is neccessary to assume a unique set of receptors for mediating the feeling of vibration. Since the intensity sensibility for the feeling of vibration is different from the intensity sensibilities for the feeling of pressure and other cutaneous sensations, it seems reasonable to suppose that the vibration sense is an independent form of sensation. Further, since the intensity-sensibility for the feeling of vibration is the same as the corresponding sensibility for the hearing of vibration, it seems plausible to suppose that the sense of hearing is a genetical development of the sense of feeling of vibration.

Incidentally, it seems that these experimental findings concerning frequency-sensibility would support the "resonance" theory of hearing and conflict with the "telephone" theory. The vibration receptors in the finger tips are not arranged in contact with, or in close proximity to, any special membrane, like the basilar or tectorial membranes in the cochlea, which can respond to vibrations selectively on the basis of physical resonance. Since the tactual nerve receptors which respond to vibration are not equipped with such a special mechanism for the differentiation of frequency, it is reasonable to anticipate a very poor frequency-sensibility for the feeling of vibration. On the other hand, since the frequency-sensibility for hearing is very good, it seems reasonable to assume some special frequency-differentiating-mechanism, as a series of tuned resonators, in the cochlea.

[20]Katz, *loc. cit.*
[21]Zigler, *loc. cit.*

VI. Comparative Study of Hearing and Feeling Vibrations

In recent years auditory studies have yielded an abundance of precise and valuable facts concerning auditory function. This precise study of auditory function has been facilitated greatly by the development of electrical circuits used in telephony. Similar types of electrical circuits, with certain changes, are equally well adapted for investigating the feeling of vibration.

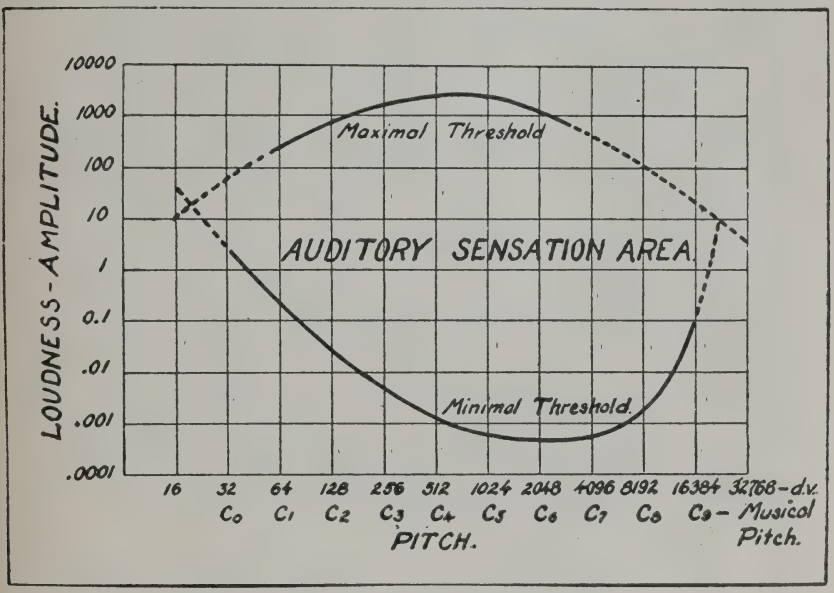

FIGURE 7

Curves Showing the Minimal and Maximal Thresholds of Loudness for Tones Throughout the Entire Audible Pitch Range. The lower curve is the frequency-sensitivity curve for hearing for the average normal. The loudness is expressed in terms of the root mean square pressure variation, in dynes per square cm., against the drum membrane. (Adapted from the curves of Fletcher and Wegel.)

In Fig. 7 are summarized many of the important auditory data obtained by Dr. Harvey Fletcher, R. L. Wegel, and others, at the Bell Telephone Laboratories. The lower curve in Fig. 7 indicates the minimal threshold of audibility as a function of frequency throughout the entire audible range. The ordinates indicate the pressure variations in the sound wave impinging upon the drum membrane, in dynes per square centimeter, required to just barely

elicit sound sensations. The abscissæ represent the frequencies of vibration of the different tones employed in the tests. Both the pressure variations, which are proportional to the amplitudes of vibration, and the frequencies of vibration are plotted on logarithmic scales. In the more important speech range, namely, from 500 to 6000 d.v., approximately .001 dyne pressure variation produces a barely audible sensation. The cochlea is most sensitive over this range. For either lower or higher frequencies, the sensitivity decreases.

The upper curve in Fig. 7 indicates approximately the maximal threshold of audition; that is, the intensity of sound which can be "felt" by the cochlea, or more probably by the drum membrane. This maximal threshold is close to the painful threshold and it can be duplicated for a given tone nearly as accurately as the minimal threshold. Fletcher states that if a sound be loud enough to produce a tickling sensation in the ear, the same sound, if impressed upon the tip of the finger, will produce a tactile sensation.

The two points of intersection of the two curves in Fig. 7 indicate the lower and upper pitch limits for hearing. Accordingly, the frequency limits for hearing are approximately 20 d.v. at the lower limit and 20,000 d.v. at the upper limit. The area between the two curves indicates the area of auditory sensation. If a simple harmonic vibration be of such a frequency and intensity that it is within this area it will be sensed as a tone of given pitch and loudness. If the vibration be of such a frequency and intensity that it is outside of this area it does not produce an auditory sensation.[22] Any point within the bounded area corresponds to a pure tone of given pitch and loudness.

The loudness sensibility of the average normal ear has already been discussed, and is shown in Fig. 6. Since the loudness sensibility is independent of the pitch, the curve in Fig. 6 was composed from the average results of measurements obtained from pure tones having frequencies of 64 d.v., 128 d.v., 256 d.v., 512 d.v., 1024 d.v., 2048 d.v., and 4096 d.v. The Fechner ratio $\Delta A/A$ decreases as the loudness increases up to an amplitude ratio of 1000. For intensities up to an amplitude ratio of approximately 100,000 the Fechner ratio has a constant value of approximately .05. At very great intensities the Fechner ratio probably increases again, although this region has not been investigated.

The pitch sensibility of the average normal ear is show in Fig. 8.[23] $\Delta N/N$ expresses the fractional change of frequency which

[22]The validity of this latter statement is questionable for very intense vibrations which are painfully loud, as they may be both felt and heard.

[23]This curve is obtained from the writer's data on the sensibility of the ear to small differences of intensity and frequency, *loc. cit.*

is just barely discernible. $\Delta N/N$ is plotted as a function of the frequency, from 64 d.v. to 4096 d.v. $\Delta N/N$ is approximately .01 for frequencies near 64 d.v. and gradually decreases as the frequency increases, approaching a constant value of about .003 at 512 d.v. The pitch sensibility, expressed inversely by $\Delta N/N$, remains at the constant value of .03 from 512 d.v. up to 4096 d.v. For frequencies above 4096 d.v. the ratio probably increases again, as indicated by the dotted portion of the curve.

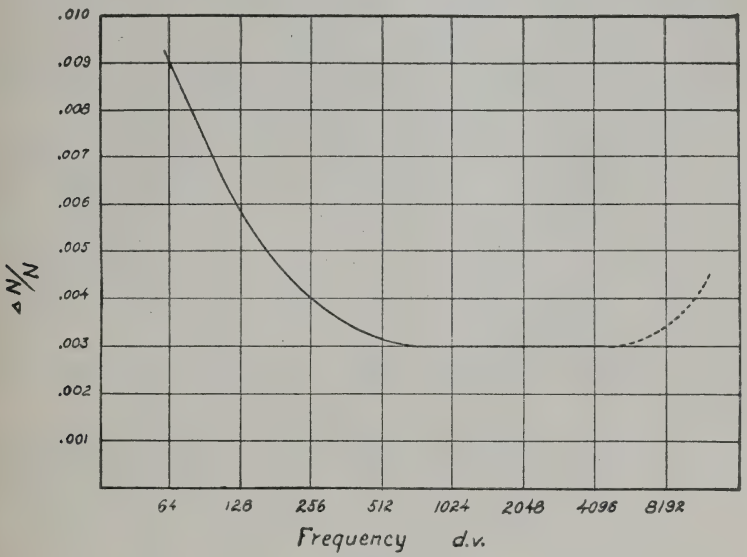

FIGURE 8

CURVE SHOWING THE FREQUENCY-SENSIBILITY FOR HEARING. The smallest noticeable fractional change of frequency, $\Delta N/N$, is plotted as a function of the frequency. Notice that for frequencies above 512 d.v. the ear can recognize a fractional change of frequency of about .003.

Using the data expressed by these last three curves (Figs. 6, 7, 8), it can be shown easily that the average normal cochlea can, for tones throughout the important speech and music range, distinguish somewhat more than 200 gradations of loudness between the minimal and maximal thresholds, and also can distinguish approximately 1500 gradations of pitch between the lower and upper pitch limits.

A comparison of these prime characteristics of audition with the corresponding ones for the sense of touch will help greatly to determine to what extent the sense of touch may be utilized as a substitute for the sense of hearing. The ear responds to harmonic vibrations

from 20 d.v. to 20,000 d.v. The range for the sense of touch is from about 16 d.v. to somewhat more than 1600 d.v., and probably as high as 4000 d.v., although the range above 1600 d.v. has not been investigated. It will be noted therefore that the lower frequency limits for both hearing and feeling of vibration are approximately the same, namely about 18 d. v., but that the upper frequency limit for hearing exceeds the upper frequency limit for feeling at least two or three octaves.

The necessary frequency range for satisfactory perception of speech is about 50 to 5000 d.v., or approximately six and one-half octaves. The sense of touch can respond to at least the first four octaves of this range. So far as the frequency range is concerned, therefore, the sense of touch is adapted to respond to at least three-fifths of the range of frequencies which make up speech.

A comparison of the frequency-sensitivities for hearing and feeling vibration indicates a number of similarities and differences. In the case of hearing the sensitivity increases with the frequency up to about 2048 d.v. and then decreases for higher frequencies. The cochlea is most sensitive for frequencies above 500 d.v., the more important portion of the speech range for the correct interpretation of speech.[24] The tactile nerves, on the contrary, are most sensitive for frequencies below 500 d.v., the less important portion of the speech range, but the range which contains most of the energy of speech.[25]

Both the auditory and the tactile nerves are extremely delicate in their response to vibratory motion. The cochlea, throughout the frequency range for which it is most sensitive, namely, from 600 to 6000 d.v., can detect a harmonic pressure variation of one-billionth of atmospheric pressure. This corresponds to a sound wave of which the amplitude is only one-fiftieth part of the diameter of a molecule, that is, about 10^{-9} cm. The tactile nerves in the finger tips can detect a harmonic amplitude variation of the order of the wave length of visual light, that is, about 10^{-4} cm. The cochlea is, therefore, on an amplitude scale, about 100,000 times more sensitive than the tactile sense, in the response to sound vibrations. These small pressure or amplitude variations at the minimal threshold, even for the sense of touch, correspond similarly to exceedingly small amounts of energy, and therefore either the hearing or the feeling of vibration can be accomplished with a small expenditure of energy in the vibrating source. Hearing, of course, is much more delicate than feeling, but both senses respond to amounts of vibratory energy which can be established very easily by electromagnetic devices. For

[24]Fletcher, H. The nature of speech and its interpretation. *J. Frank. Inst.*, June 1922.

[25]Crandall, I. B., and MacKenzie, D., *Phys. Rev.*, 1922, **19**, 221-232.

the tactile sense, the amplitude of the sound waves should be amplified about 100,000 times the amount required for hearing.

The cochlea has a wide range of intensity accomodation; the loudest sounds it can tolerate, in the frequency range from 256 to 4096 d.v., have an amplitude of approximately one million times the minimal-threshold-amplitude. For very low and very high pitched sounds the intensity range becomes much narrower, as can be seen in Fig. 7. In the case of feeling of vibration, owing to experimental limitations, measurements for only two frequencies were obtained at the maximal or painful limit, but these measurements indicate that for frequencies of 32 to 64 d.v. the most intense vibrations the finger tips can tolerate have an amplitude of from about one thousand to five thousand times the minimal threshold amplitude. The range of intensity accommodation to vibratory motion, possessed by the tactile nerves, therefore, is slightly more than one-half of the intensity range enjoyed by the cochlea. (This, of course, is based upon a logarithmic scale.) Throughout its limited range, the sense of touch to a vibrating body is about as good as the cochlea is in the response to small differences of intensity. Throughout this limited range, therefore, the sense of feeling vibration is as effective as the sense of hearing for perceiving the intensity variations associated with speech, provided, of course, the speech has been sufficiently amplified and appropriately transformed into mechanical vibrations which can be felt with the finger tips.

It will be recalled, by referring again to Figs. 5 and 6, that the intensity sensibilities for both hearing and feeling of vibration are essentially identical. That is, either the cochlea or the tactile nerves can just barely distinguish a fractional change of amplitude of approximately one-tenth at low intensities and one-twentieth at high intensities. The agreement of the data for hearing and feeling differences of intensity of a vibrating body strongly suggests a certain functional resemblance of the auditory and tactile nerves. It would seem reasonable to assume that the same type of intensity-differentiating-mechanism possessed by the cochlea is also possessed by the tactile nerves. According to the generally accepted neurone theory, nervous energy is made up of certain quanta of energy. It is the number of nerve fibres excited that determines the intensity of the sensation, each fiber communicating its characteristic quantum of nervous energy. Hence it seems reasonable to expect the same intensity sensibility for either hearing or feeling vibrations; for if there is a uniform distribution of nerve fibers in the cochlea and also in a local area of the skin, a given fractional increase in the amplitude of vibration of the stimulating mechanism will produce a corresponding increase in the activated area of nerve fibers in

either the cochlea or the skin and consequently in the number of stimulated nerve fibers.

Finally, by extending the comparison of hearing and feeling of vibration to the frequency-differentiating-mechanisms possessed by these two senses, it will be recalled that the cochlea can just barely recognize a fractional change of the frequency of .01 around 64 d.v. and a fractional change of .003 for frequencies above 512 d.v. On the other hand, the frequency-sensibility for feeling is very crude. In fact it is sometimes difficult to appreciate any difference between two harmonic vibrations which differ as much as an octave. On the average, the results of this investigation indicate that the tactile nerves can just barely recognize a fractional change of the frequency of .15 to .30. Therefore the frequency sensibility of the hearing sense is from 15 to 100 times better than the frequency-sensibility of the feeling sense. This would indicate that the tactile nerves, compared with the cochlea, are poorly endowed with the analytical equipment required for the appreciation of "pitch" or "pitch" differences. This analytical function, it is generally believed, is quite indispensable in the hearing of speech and wholly indispensable in the hearing of music.

To summarize, the physical characteristics of the sense of touch to vibration which are pertinent to the problem of interpreting speech vibrations are as follows: (1) The sense of touch can appreciate approximately three-fifths of the frequency range employed in speech, the deficiency being in the range of the higher frequencies. (2) The sense of touch is most sensitive to those frequencies which comprise most of the energy of speech, that is, frequencies below 500 d.v., but is relatively less sensitive to those frequencies which comprise the most important portion of the speech range, that is, frequencies above 500 d.v. (3) The sense of touch, throughout its intensity range, is as good as the sense of hearing for discriminating differences of intensity in speech vibrations provided the vibrations are communicated suitably to the tactile nerve-endings in the skin. (4) The sense of touch is poorly suited to the frequency-differentiating requirements for the proper hearing of speech.

It is obvious, therefore, that the sense of touch possesses some of the physical characteristics required for "hearing" speech vibrations, but that it does not possess certain other characteristics, the greatest deficiency occurring in its incapability for recognizing absolute frequency or small differences of frequency. However, the sense of touch does possess one outstanding characteristic which should prove of sufficient value to warrant its use for the reception of speech: the capability for appreciating small differences of intensity of a vibrating body, especially throughout the frequency range which carries most of the energy of speech. The sense of touch can

recognize at least fifty different gradations of intensity of vibration, so that this analytic function of the sense of touch may be trained sufficiently to recognize the various shapes or forms of the vibratory "patterns" or "envelopes" of the characteristic speech sounds, provided the speech sounds have been sufficiently amplified and suitably mediated to the tactile nerves. In this connection, it will be appropriate to examine some oscillograms of typical speech sounds. In Figs. 9 to 13 are reproduced some oscillograms of the names of the days of the week. These were obtained with the use of an oscillograph which was developed and constructed by Mr. L. P. Delsasso of the Department of Physics. The names of the week were spoken into an electromagnetic telephone transmitter; the voice currents thus produced were amplified with an amplifier; and these amplified voice currents were then impressed upon the vibrator of the oscillograph. The speed of the film upon which the records were obtained was uniformly ten inches a second. The names of the days of the week were chosen because these were used by Professor Gault as one group of words in his experiments on Tactual Interpretation of Speech.[26]

Since the shapes or patterns of these oscillograms are of prime interest in this study, the envelopes of the oscillograms have been inked with a pen, indicated by the dotted lines. No attempt was made to trace very small changes in the shapes of the curves, since

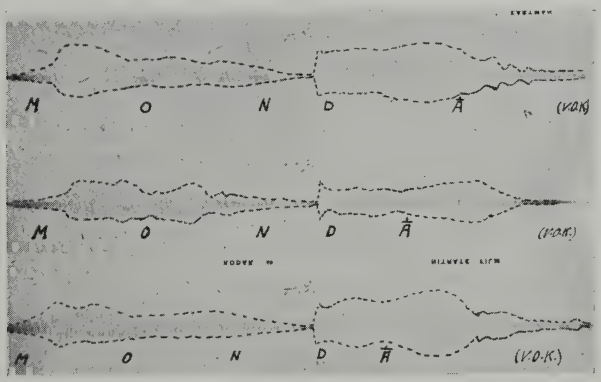

FIGURE 9

OSCILLOGRAMS SHOWING THE ENVELOPES, OR THE INTENSITY VARIATIONS OF THE SAME WORD, "MONDAY," SPOKEN SUCCESSIVELY THREE TIMES BY THE SAME SPEAKER. These were obtained to indicate the degree of accuracy of reproducibility of the same word spoken by the same speaker.

[26] *Loc. cit.*

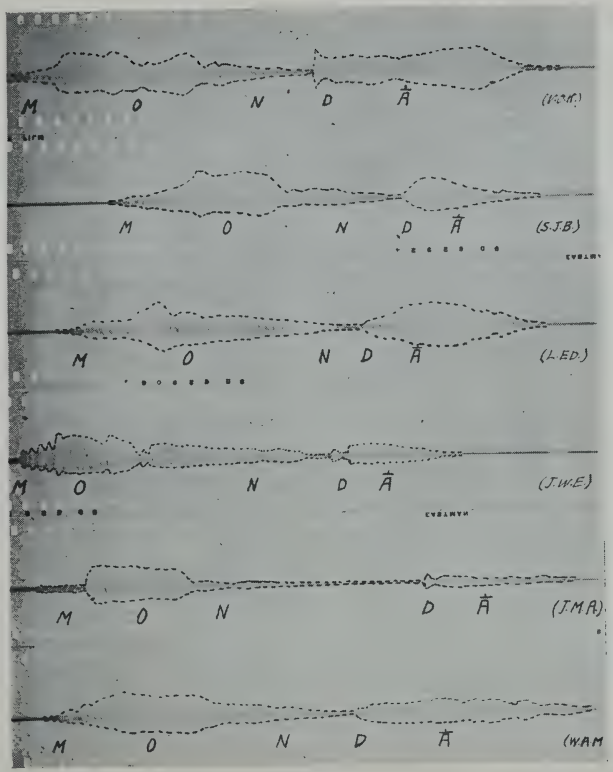

FIGURE 10

THE SAME WORD, "MONDAY," SPOKEN BY SIX DIFFERENT MALE SPEAKERS. These indicate the degree of accuracy

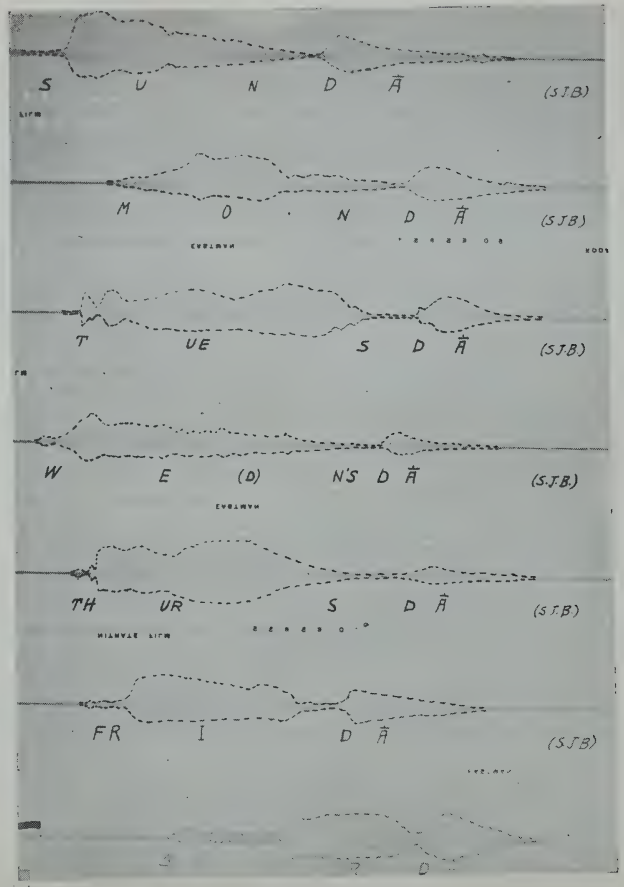

FIGURE 11

THE ENVELOPES OF THE NAMES OF THE DAYS OF THE

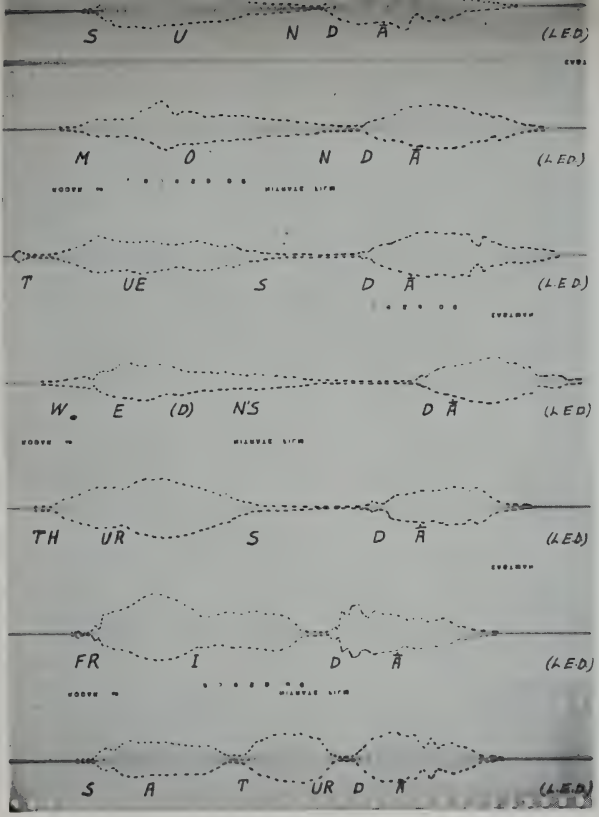

FIGURE 12

THE ENVELOPES OF THE NAMES OF THE DAYS OF THE WEEK SPOKEN BY A BARITONE VOICE. (Compare with Figs. 11 and 13.)

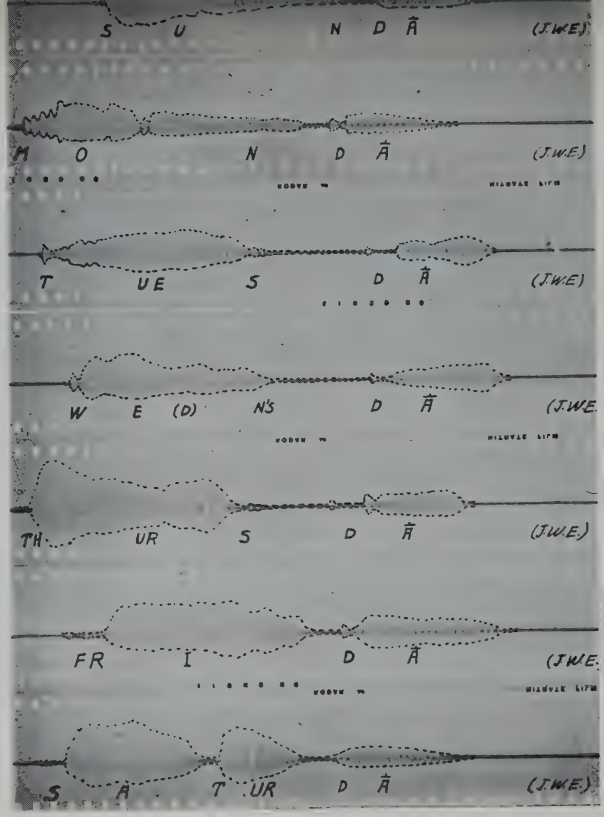

FIGURE 13

THE ENVELOPES OF THE NAMES OF THE DAYS OF THE WEEK SPOKEN BY A BASS VOICE. (Compare with Figs. 11 and 12.)

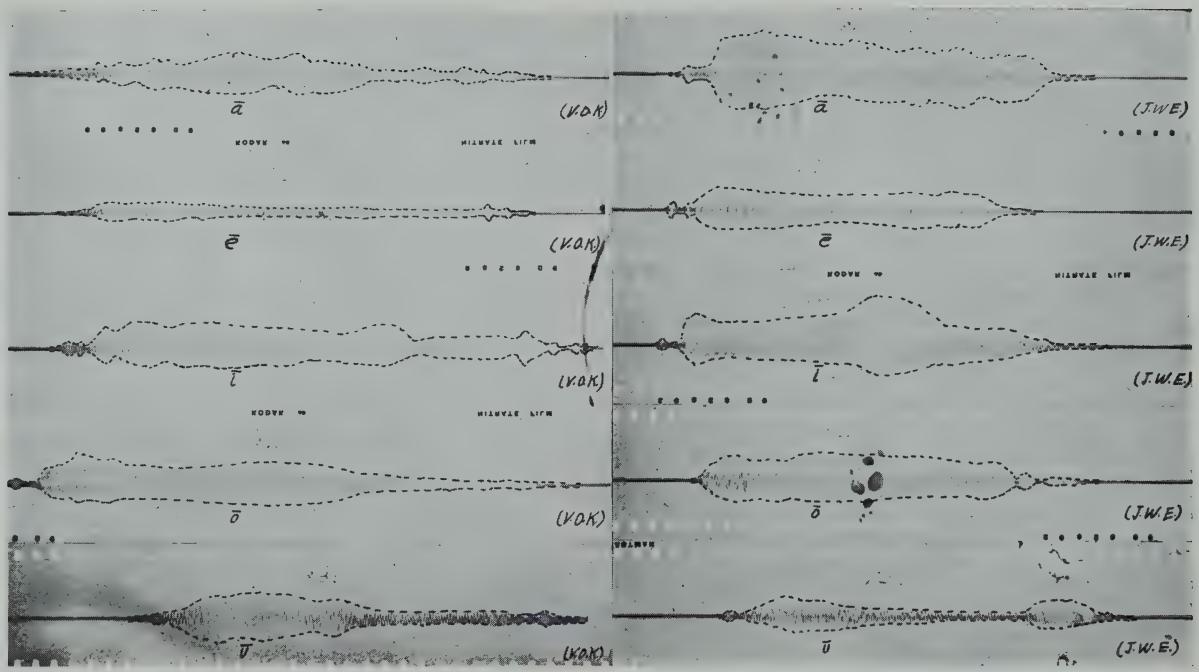

FIGURE 14

THE ENVELOPES OF THE LONG VOWEL SOUNDS. The ones on the left were spoken by a tenor voice; the ones on the right by a bass voice.

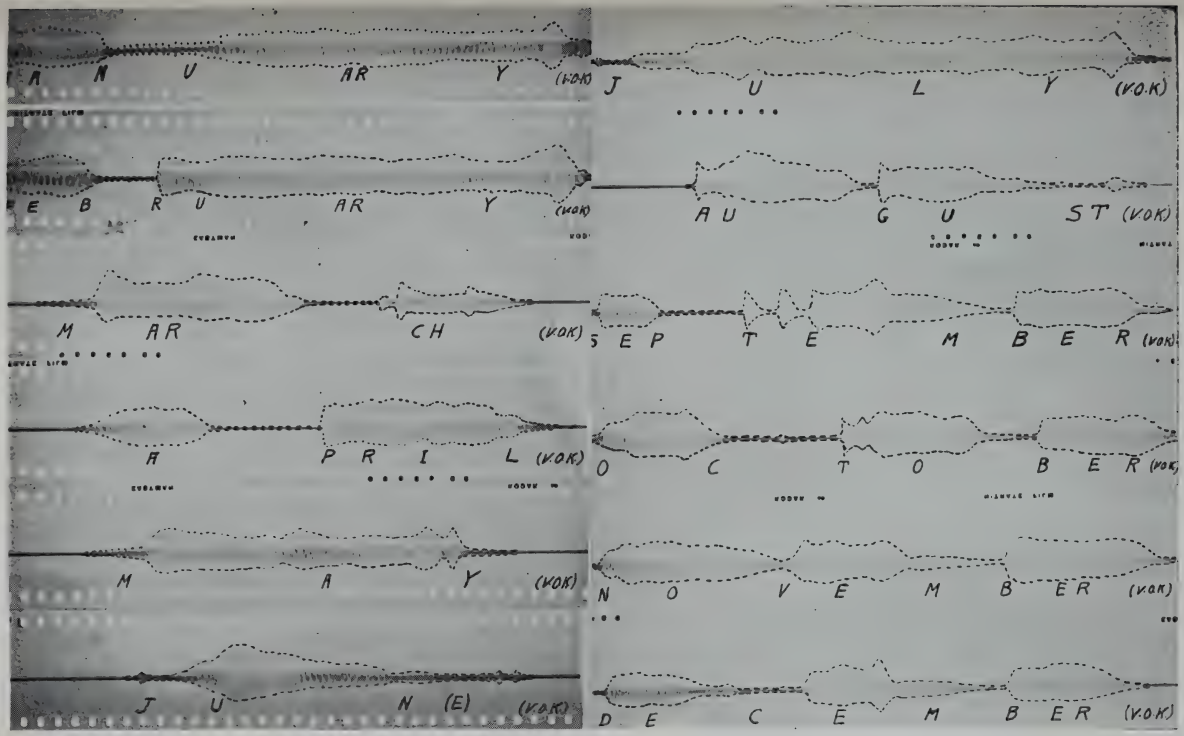
A N U AR Y (VOK)
J U L Y (VOK)
E B R U AR Y (VOK)
AU G U ST (VOK)
M AR CH (VOK)
SEP T E M B E R (VOK)
A P R I L (VOK)
O C T O B E R (VOK)
M A Y (VOK)
N O V E M B E R (VOK)
J U N (E) (VOK)
D E C E M B E R (VOK)

FIGURE 15

THE ENVELOPES OF THE NAMES OF THE MONTHS OF THE YEAR SPOKEN BY A TENOR VOICE. Note that March appears as a two-syllable word whereas July appears as a monosyllabic word.

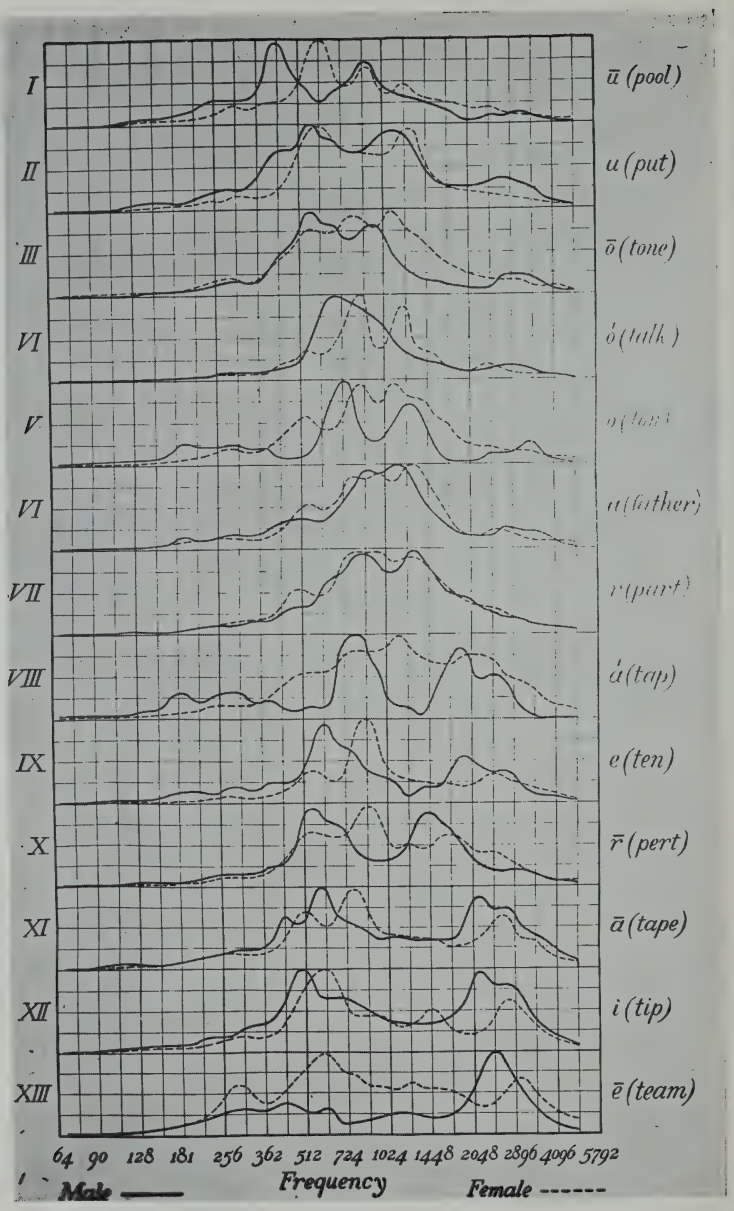

FIGURE 16

CURVES SHOWING THE DISTRIBUTION OF ENERGY AS A FUNCTION OF FREQUENCY FOR CERTAIN VOWEL SOUNDS. (Taken from a publication of I. B. Crandall.)

many of these are probably attributable to the sound interference pattern surrounding the transmitter. Rather, the attempt was made to indicate only the general characteristic shapes of the curves.

Fig. 9 shows the oscillograms of the word "Monday" spoken three times in succession by the same speaker (V. O. K.). These were selected as typical of the degree of accuracy of reproducibility of the pattern for a particular word, spoken by the same speaker. It will be noted that there is a close similarity between these three patterns, although the accuracy of reproducibility is not all that might be anticipated. The duration and time intervals of the corresponding parts of the three oscillograms are in good agreement. The beginnings of both syllables (*Mon* and *da*) are strikingly alike. The endings of the first syllable (*Mon*) are similar, but the endings of the second syllable (*da*) are quite different.

Fig. 10 shows the oscillograms of the word "Monday" spoken by six different speakers, all males. It will be noted that many of the similarities of Fig. 9 have disappeared. The durations and time intervals of the corresponding parts of the six oscillograms are approximately alike, and the beginnings of the first syllable (*Mon*) are generally similar, but the steady state portions and the endings of both syllables are peculiarly different.

Figs. 11, 12, and 13 show the oscillograms of the names of the days of the week, spoken by three different males (S. J. B., L. E. D., and J. W. E.). Directing the attention to one of these figures at a time, it will be noted that the envelopes of the seven days of the week exhibit sufficiently different characteristics to anticipate a good degree of accuracy in tactual interpretation, so long as the same speaker pronounces the words. "Sunday" and "Monday," as one would anticipate, bear the closest resemblance to each other; and in Professor Gault's tests[27] these two words are mistaken one for the other with the greatest frequency. Thus, Sunday was misinterpreted 74% of the total number of times it was called, and Monday was misinterpreted 52% of the total number of times it was called. Saturday, as one would also anticipate, possesses the most unique and distinguishable characteristics. This word was misinterpreted only 9.6% of the total number of times it was called in Professor Gault's tests. From an inspection of the oscillograms of the days of the week, one would expect even greater accuracy than this for the word Saturday.

Comparing Figs. 11, 12, and 13 it will be noticed that the three different speakers have outstanding differences. Thus it would appear that the patterns of the same word spoken by different

[27] *Loc. cit.*

speakers may differ among themselves as much as the patterns of different words spoken by the same speaker. It would be of interest to test this fully in the experiments of Professor Gault.

Fig. 14 shows the oscillograms of the five long vowels (a, e, i, o, u) spoken by two different males (V. O. K. and J. W. E.). Fig. 15 shows the oscillograms of the months of the year spoken by V. O. K. Note that March appears to have two syllables, the second one coming from the "ch," and that July appears to have only one syllable. The "ch" in March, however, is a high frequency vibration, mostly above the tactual frequency range, and probably would give little or no tactual impression. These and other characteristics of the oscillograms for the months of the year explain many of the observer's errors of tactual interpretation in Professor Gault's tests with this series of words.[28]

The envelopes of the oscillograms in Figs. 9 to 15 show principally the intensity variations which characterize the several selected words and vowels. These intensity variations include such factors as the rate of growth and decay of intensity, the duration of steady or periodic states, and the intervals of relatively no vibration, all of which contribute characteristics which should be helpful in the tactual interpretation of speech. As the envelopes of the oscillograms show, the characteristics of a given word spoken by different speakers are not so unique as one might anticipate or hope. This, it would appear, constitutes a real difficulty in the problem of teaching the deaf to interpret speech tactually. Of course, this same "difficulty" exists in the case of hearing, but the cochlea possesses a most excellent frequency-differentiating-mechanism, which is a prime factor in the recognizing of speech sounds.

It is possible that the frequency-differentiating-mechanism of the sense of touch to vibration, even with its low degree of sensibility, also plays a part in the tactual interpretation of sound vibrations. The energy distribution among the various frequencies which comprise the sounds of speech seems to be a dominant factor which characterizes the sounds of speech. This is demonstrated by the artificial production of vowel sounds which can be accomplished by appropriate electro-acoustic circuits.[29] The energy distribution of the frequencies which comprise certain speech sounds are shown in Fig. 16 which is taken from the excellent work of I. B. Crandall of the Bell Telephone Laboratories.[30] It will be noticed that there are in general two prominent maxima for each speech sound analyzed,

[28]The interested reader should compare these oscillograms with Professor Gault's results. (See *J. Appl. Psychol.*, 1926, **10**, 85-87.)

[29]See for example Stewart, J. Q. An electrical analogue of the vocal organs. *Nature*, September 2, 1922.

and that the location of these maxima, and especially their separation, is characteristic of each sound. In most cases these separations are sufficient to be distinguished by the sense of touch to vibration. It is possible therefore that the discrimination of frequency and differences of frequency may contribute to the tactual interpretation of speech. However, the very poor frequency-sensibility of the tactile nerves throws considerable doubt on the usefulness of this factor as an aid to tactual interpretation of speech.

The significant conclusion warranted by this preliminary investigation of the tactual interpretation of sound vibrations is that the tactual sense does possess certain analytic functions which, if adequately trained, should be serviceable for the interpretation of speech. This, of course, has already been borne out by the experiments of Professor Gault, and is also confirmed by the success in the tactual interpretation of speech of such well-known cases as Helen Keller, Willeta Huggins and others. However, the tactual sense is extremely limited in certain required analytic functions, especially in its ability to appreciate absolute frequency and small differences of frequency. The tactual interpretation of music therefore would be almost void of melody or pitch coloring. The rhythm, the time intervals and much of the expression would be appreciated as well as in hearing, but only the larger intervals of pitch would have any significance. The tactual interpretation of speech, on the other hand, seems to encounter less severe difficulties.

It should be emphasized that tactual interpretation of speech is not a panacea for the deaf, but it does offer them a useful aid to "hearing" speech. Perhaps its greatest usefulness would be attained in conjunction with lip-reading. The vowels, which constitute most of the energy of speech and which are made up largely of low frequencies, could be interpreted tactually, and the consonants which comprise a relatively small amount of the energy of speech and which are made up largely of high frequencies, could be interpreted visually. In this way the tactual sense would be utilized to respond to those speech-sounds for which it is most sensitive, namely the vowel sounds, and lip-reading would be utilized to interpret those sounds of speech which are characterized by the most conspicuous configurations of the lips, namely the consonant sounds. The tactual sense, therefore, offers a ray of hope to those who are interested in alleviating deafness. The problem of designing a suitable instrument for mediating the sound vibrations to the tactile nerves should not present serious difficulties, and probably can be worked out in an entirely satisfactory manner by telephone engineers.

[30]Crandall, I. B. Sounds of speech. *Bell System Technical Journal*, Oct. 1925, 4, 586.

Surely the preliminary data on the sensitivity characteristics of the sense of touch to sound vibrations and the success of Professor Gault's experiments would warrant a continuation of the experimental studies in training the deaf to interpret the tactual impressions of speech.

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Los Angeles, California

TACTUAL PATTERNS FROM CERTAIN VOWEL QUALITIES INSTRUMENTALLY COMMUNICATED FROM A SPEAKER TO A SUBJECT'S FINGERS*[1]

From the Vibro-Tactile Research Laboratory

ROBERT H. GAULT AND GEORGE W. CRANE

In numerous connections hitherto we have instrumentally communicated the vibrations that correspond to speech to the finger tips of both hearing and deaf cases so that distinctions can be observed not only amongst words and sentences but amongst the vocal elements of words. Elsewhere we have described the tactual patterns of certain vowel and diphthongal qualities as they are obtained upon the ball of a thumb from the diaphragm of a single unit radio-earpiece.

It is most desirable in this connection to enlarge the criteria (in other words, the differences amongst patterns of spoken language and its elements) to the utmost. This requires the development of unique instrumentation and methods of stimulation to bring out as many tactual distinctions as possible among the forms of speech. It is all the more important to provide as many criteria of the forms of speech as possible because we are developing a tactual method for establishing a feeling for language among the deaf; a method that will be of practical use in connection with receiving and interpreting speech; in developing control of vocal expression; in enhancing the enjoyment of spoken discourse; and at the same time, a method that will even facilitate silent reading on the part of the deaf.

We were looking for a scheme for multiplying these distinctions when we conceived of grafting a mechanical ear upon the skin: a device that will analyze the sounds of speech according to their frequencies. Bell Telephone Laboratories have designed and built such an instrument for our use; a multiple unit teletactor. It includes five magnets and five moving parts or vibrators—one for each finger and the thumb of a hand. A filter is used in connection with the instrument. The lower frequencies (pitches) that occur in human speech (up to 250 double vibrations a second) operate a unit against the thumb. Frequencies of 250-500 work against the index finger; 500-1000 against the middle finger. The third finger receives stimuli of a vibrational rate of 1000-2000 a second while the little finger receives high rates of 2000 and above.

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[1]These experiments are being conducted under the auspices of the Carnegie Institution of Washington.

When all fingers are in their proper places upon the instrument, and when long *e* is spoken into the system, it is obvious that whether this finger or that one is stimulated, or indeed whether several are simultaneously impressed, will depend upon the pitch of the speaker's voice: upon its fundamental and its higher frequencies. Thus, if our speaker's voice should correspond exactly with the Mean Fundamental Frequency, the Mean Low Characteristic Frequency and the Mean High Characteristic Frequency as found by Crandall[3] (136 d.v., 296 d.v., and 2987 d.v., respectively—male voice), long *e* should be felt upon the thumb and the index finger: improbably upon the little finger. We say "improbably upon the little finger" for the reason that the highest rate of vibration that can be sensed tactually in our conditions is 2600 d.v. This is the meaning of the tactograms[4] that we established in the Vibro-Tactile Research Laboratory during April and May, 1927, which, however, may not be the final word. One subject in our laboratory, for instance, a deaf case, appeared to sense a vibration rate of 3000 d.v. But we had no opportunity to check up. In our experiments relating to tactograms the stimuli were produced by an oscillator and the situation was arranged so that the subject could give maximal attention to the stimulation, upon the skin or upon a finger nail. It is altogether probable that heights that are attainable in such conditions can not be reached in other circumstances in which more areas than one are being stimulated simultaneously by the human voice, or in rapid succession.

To return to the tactual patterns—such as we have described to the present time. We will first describe the laboratory situation and the modus operandi by which the patterns have been discovered.

The subjects were two normal adults who are interested in the work as a problem in psychology and education. While the experiments were in progress they were in a situation in which they could not hear either the speaker's voice or the action of the teletactor.

The stimuli that were chosen for this purpose are as follows: *e*(eel), *er*(pert), *oo*(boot), *o*(ore), *ah*(father), *aw*(all). The subject placed his thumb upon the appropriate contact. The speaker at the microphone, thirty-five feet distant and beyond two closed doors, selected a stimulus (the subject was ignorant of the order of his choices), flashed a signal before the subject and pronounced into the microphone. If the subject felt a vibration upon his thumb he indicated it in the proper place upon a chart. Thereupon he applied his index finger at its proper place, and each other finger in turn. The speaker held each vowel quality about four seconds and repeated it again and again until the subject indicated by a buzzer

[3] *Bell System Technical Journal*, **4**, 1925, 586-626.

[4] See "'Hearing' through the Sense Organs of Touch and Vibrations." *J. of the Franklin Inst.*, **204**, 1927, pp. 356 and 357.

that he was ready for another stimulus. In the meantime the subject had indicated on a suitable chart whether he caught the stimulus upon his thumb, index, middle, third, and little finger respectively in succession. Not only so but his report for each finger indicated that he could feel it upon that finger "very distinctly," "distinctly," "indistinctly" or not at all. Signs in the chart like the following: (1x, 2/4—) mean that the stimulus was felt very distinctly upon the thumb, distinctly upon the index finger, and indistinctly on the fourth digit (third finger). It was not felt at all upon the middle finger (third digit) and not at all upon the fifth digit or little finger. The signs, taken together, indicate a "pattern."

One of the two subjects (L) had never seen the multiple unit teletactor till these experiments began and had never practised even on the single unit instrument. The other subject (G) had had no practice whatever on the multiple unit device but he had had very much of it in relation to the single unit one.

The description of patterns obtained by means of the contact of one finger at a time varies in only a minimum degree for a given subject from session to session. Variation between subjects is small excepting that one may consistently adopt a higher standard for "distinctness" e. g., than the other. So far as variations occur in the case of a given subject from day to day they are generally due to the same variation of standards.

This uniformity is justification for desisting from long continued practice on the one-finger-at-a-time determination of tactual patterns. Indeed, in view of the purpose of this work in the large, it is of very great importance to know what subjects in the situation we have described can do at the first onslaught.

Table I shows the data just as each subject brought them from the reaction room.

Opposite *e* 7x, under 1, indicates that G felt *e* seven times in succession upon the thumb and always very distinctly. Under 3-, 7,0 indicates that seven times in succession *e* was *not felt* upon the third digit or middle finger. The figures under 4 indicate that once G reported *e* as very distinct on the fourth digit, once indistinct, once distinct, and four times as undetected.

On the extreme right G's pattern for *e* is designated as follows: 1x, 2–; i. e., very distinct on the thumb and indistinct on the index finger.

In all the patterns on the right the signs of distinctness are associated with the digits 1, 2, 3, etc., to indicate how the vowel quality in question feels upon the thumb and fingers respectively. Thus the pattern for *er* reported by G is: 1x, 2/, 3–, 4x. This means that *er* is very distinct on the thumb, distinct on the index finger, indistinct on the middle finger, and very distinct on the fourth digit.

TABLE I

DATA CHART, SUBJECTS G AND L
(ONE FINGER AT A TIME)

	1		2		3		4		5			
	G	L	G	L	G	L	G	L	G	L	Patterns	
e	7x	4/	7–	4–	7,0	4,0	1x 1– 1/ 4,0	4–	1– 6,0	4,0	G L	1x, 2– 1/, 2–, 4–
er	6x	4/	4/ 2–	4–	2/ 4–	1– 3.0	6x	4x	4x 2,0	4,0	G L	1x, 2/, 3–, 4x, 5– 1/, 2–, 4x
oo	3x 1– 2/	4/	1/ 5–	3– 1/	3,0 1/ 2–	3,0 1–	3x 3/	2– 1/ 1x	6,0	4,0	G L	1/, 2–, 4/ 1/, 2–, 4/
o	4/ 2–	4/	3/ 3x	4/	3,0 4/	3– 1,0	6x	4/	1– 5,0	4,0	G L	1/, 2/, 4x 1/, 2/, 3–, 4/
ah	6x	4/	6–	4–	3– 1/ 2x	2– 2/	6x	2x 2/	5– 1,0	4,0	G L	1x, 2–, 3/, 4x, 5– 1/, 2–, 3/, 4/
aw	1– 5/	4/	4– 2/		3,0 1/ 2x	4/	6x	3x 1/	4– 2,0	4,0	G L	1/, 2–, 3–, 4x, 5– 1/, 2–, 3/, 4x

Key: The figures 1, 2, 3, 4, 5, at the top indicate thumb, index finger, etc., respectively. G and L designate the subjects.

In the chart:

x	means	very distinct
/	"	distinct
–	"	indistinct
0	"	absent

L reports the pattern for the same quality thus: 1/, 2–, 4x, which means that it is distinct on the thumb, indistinct on the index finger, and very distinct on the fourth digit.

Now the important question is: How will these patterns appear *when all the fingers are simultaneously upon their contacts and when two or more of them are being stimulated at the same time*? If the most distinct stimulation should drown out all others obviously our multiple unit teletactor is of no use and we should return at once to the single unit device analogous to the radio-ear-piece.

It is not difficult to test this question. The method we pursued is as follows: the subject seated himself in the reaction room where he could not hear the experimenter's voice and in conditions in which he could not hear the action of the teletactor. He placed

each finger and the thumb of his left hand upon its appropriate unit. At his right hand was a chart upon which he could make his record as to what fingers were being stimulated in a given instance and in what degree.

The experimenter from his distant place flashed a signal and pronounced into his microphone one of the six vowel qualities, and gave the subject about two seconds in which to make his record, signalled and pronounced another until the subject had received ten stimulations, and had written as many patterns. He was always in ignorance as to the stimuli excepting as he recognized the tactual pattern as a sign of *e* or what not. After ten stimulations the subject learned from the experimenter what the order had been and thereupon another and another and another series of ten was given in the same manner. The subject's records afford a basis for describing patterns when all fingers are in simultaneous contact.

Table II shows the total number and character of judgments made by subject L in her first three sessions, when all fingers and the thumb are on their appropriate contacts.

TABLE II

SUBJECT L. ALL FINGERS AND THUMB IN CONTACT

	1	2	3	4	5	Patterns
e	26/	24– 2,0	26,0	23,0 3–	26,0	1/, 2–
er	34/	34–	32,0 2–	33x 1/	34,0	1/, 2–, 4x
oo	33x	31– 2/	27,0 5– 1/	22– 7/ 4,0	33,0	1/, 2–, 4–
o	26/	23– 3/	15– 6,0 5/	14/ 12–	26,0	1/, 2–, 4–
ah	31/	29– 2,0	13/ 16– 2,0	24/ 3– 4x	30,0 1–	1/, 2–, 3–, 4/
aw	29/ 1–	30–	19/ 8– 2,0	24/ 3x 2–	29,0 1–	1/, 2–, 3/, 4/

Interpretation of Table II: In the course of all tests *e* was 26 times reported as *distinct* on the thumb. On the index finger it was 24 times reported as *indistinct* and twice it was not felt at all, etc.

TABLE III

SUBJECT G. ALL FINGERS AND THUMB IN CONTACT

	1	2	3	4	Patterns
e	25/ 4x	14– 15,0	29,0	29,0	1x, 2–
er	27/ 1x	18– 10,0	26,0 2–	18x 10/	1/, 2–, 4x
oo	26/ 2x	16– 13,0	25,0 4–	14,0 12– 3/	1/, 2–, 4–
o	23/ 1x	14– 3,0 6/	13,0 4– 1/	1x 15/ 4,0	1/, 2–, 4/
ah	2x 27/	23– 6,0	17– 12,0	15/ 8x 4– 2,0	1/, 2–, 3–, 4/
aw	2x 23/	22– 4,0	12,0 14–	14x 2–	1/, 2–, 4x

To facilitate comparison the patterns are brought together in Table IV.

TABLE IV

A. ONE FINGER AT A TIME

G	L
e—1x, 2–	*e*—1/, 2–, 4x
er—1x, 2/, 3–4x, 5–	*er*—1/, 2–, 4x
oo—1/, 2–, 4/	*oo*—1/, 2–, 4/
o—1/, 2/, 4x	*o*—1/, 2/, 3–, 4/
ah—1x, 2–, 3/, 4x, 5–	*ah*—1/, 2–, 3/, 4/
aw—1/, 2–, 3–, 4x, 5–	*aw*—1/, 2–, 3/, 4x

B. ALL FINGERS SIMULTANEOUSLY

G	L
e—1x, 2–	*e*—1/, 2–
er—1/, 2–, 4x	*er*—1/, 2–, 4x
oo—1/, 2–, 4–	*oo*—1/, 2–, 4–
o—1/, 2–, 4/	*o*—1/, 2–, 4–
ah—1/, 2–, 3–, 4/	*ah*—1/, 2–, 3–, 4/
aw—1/, 2–, 4x	*aw*—1/, 2–, 3/, 4/

Several things stand out in Table IV:

1. When all fingers are in contact the subjects recognize no stimulation of the fifth finger which receives vibrational rates of 2000 and above.

2. There is a great paucity of recognizable stimulation upon the middle or third digit finger,—500 to 1000 d.v.

3. On the whole the thumb and fourth digit are most distinctly impressed—up to 250 d.v. and 1000 to 2000 d.v. respectively.

4. The index finger—250 to 500 d.v.—is usually indistinctly stimulated.

5. An inspection of the patterns indicates that if there are no factors but clear localization involved certain vowel qualities will be distinguished from other with some difficulty.

By pairing each vowel quality in the list with every other, and trying to distinguish them by their tactual qualities, we have arrived at the following order of increasing difficulty of discrimination of pairs: (1) *e-er; e-o; e-ah; e-aw; er-oo; er-o; er-ah; er-aw; oo-aw; o-aw;* (2) *oo-ah;* (3) *e-oo; o-ah;* (4) *ah-aw.* Members of pairs in the first group were invariably distinguished in 20 preliminary trials. *oo-ah* were distinguished in 18 out of 20 trials; *oo-o* in 14 cases; *e-oo* and *o-ah* in 12 cases and *ah-aw* in 8. Preparatory to each series of 10 trials the speaker pronounced the members of the pair twice in in a known order. But one subject (G) was employed in this connection, and he had had only such practise with the multiple unit teletactor as was involved in obtaining the patterns already reported.

Drill will undoubtedly remove some of these difficulties. Indeed, it became evident to the subjects in the course of the present work that when distinctions cannot be made on the basis of describable patterns they can be made, in some instances certainly, on the ground of a certain indefinable extensiveness or massiveness of stimulation.

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PURSUIT LEARNING UNDER THREE TYPES OF INSTRUCTION*

From the Psychological Laboratory of the Ohio State University

SAMUEL RENSHAW AND DOROTHY K. POSTLE[1]

This paper reports an experiment in which three groups of five subjects each, working under different instructions, learned to operate a pursuitmeter. The design of the instrument and the task it imposes upon the subject make it particularly suited for experiments in which the subject's own implicit language responses are factors in learning, as will be shown later in this paper.

The results indicate that putting on the language habits first is detrimental to manipulatory learning except in cases where it can set the reactor for a subsequent change of response in the *direction* of movement, in *time* or ordinal series, or in *counting* responses such as are involved, for example, in the multiple-choice type of experiments.[2] The group of our subjects who received the most complete instruction as to how to get the most effective performance made strikingly little improvement as compared with the two groups whose only instruction was, in the one case, to keep the instrument running continuously and in the other to try to discover as quickly as possible the most effective method of work. Bain,[3] Watson,[4] Bloomfield,[5] Weiss,[6] and others have asserted that language may be regarded as a substitute for objects and for manipulatory acts. Consequently, to some extent at least, we should be justified in expecting that the foreshortening of the period of the initial precurrent or exploratory trials in learning should stand in inverse relationship to the extent to which the verbal substitutes for the acts are furnished the subjects in the instruction. Especially is this true where the subjects implicitly

*Received for publication by Albert P. Weiss of the Editorial Board, July 6, 1927.

[1]The writers are indebted to Miss Bertha Roth and to Mr. M. C. Hargraves for assistance in conducting the experiment.

[2]Language 'favors' learning in such cases as tracing a maze pattern in which the series of movements can be represented by 'left-right,' etc., or in problem boxes where the sequence of levers, latches, etc., are operated in sequential order, or where the 'rule' of a multiple choice order can be generalized as 'second key from the left if the number of the exposed keys is even.'

[3]Bain, A. The Senses and the Intellect.

[4]Watson, J. B. Behaviorism. N. Y.: Norton, 1923.

[5]Bloomfield, L. An Introduction to the Study of Language. N. Y.: Holt, 1914.

[6]Weiss, A. P. A Theoretical Basis of Human Behavior. Columbus: R. G. Adams, 1925.

run through both the subvocal and manual activities either concurrently or in alternation. Thus if we increase the detail of the instruction as to *what* to do and *what* to look out for, the slope of the learning curve for some relatively simple manipulation should become steeper and steeper with the greater and greater replacement of 'practice' by implicit word cues within the limits imposed by the fact that many serial acts have components for which there are no communicable language equivalents.

The Nature of the Pursuit Task

Miles[7] has shown "that the nature of the pursuit task not only requires quick reaction but also that the movements be executed in a fairly definite pattern. A reaction is usually only the beginning of coordinated movements toward some end or series of such coordinations, and probably in most instances in practical life the adequacy with which the series of coordinations is carried through is fully as important, if not more so, than the mere matter of speed in initiating them. Especially would this appear to be the case in movements for compensating, directing, aiming, or otherwise tending any moving object, where the pace and pattern are not set entirely by the subject himself."

The mechanical features of the apparatus used in this experiment have been described elsewhere.[8] The problem which its operation presents to the subject may be described briefly as follows: Seated before a small table on which there is an upright screen which conceals all working parts, the subject sees before him a ¾ inch strap-iron arm which extends through the screen and which bears upon its top surface a one-inch vertical piece of fiber tubing. This is referred to as the 'bead.' In the center it carries a circular 2 mm. electrode. The top surface of the bead is slightly rounded so that a hammer-shaped stylus, similarly constructed, must be kept in horizontal contact (within + or -9^0 of arc) as the 'bead' moves about upon its plate glass bed. Its rate of motion and direction are in continuous change, being driven by a series of gears of different pitch diameters. Although the cycle of the 'bead' repeats each three minutes, the fact that not one of a hundred subjects who have operated the instrument has discovered this fact indicates that its pattern is too complex for verbal representation.

Failure to maintain contact in the local stylus-bead circuit immediately stops the instrument and a small signal buzzer sounds until

[7]Miles, W. R. The pursuitmeter, an apparatus for measuring the adequacy of neuromuscular coordination. *J. Exper. Psychol.*, 1921, **4**, 77.
[8]Renshaw, S., and Weiss, A. P. Apparatus for measuring changes in bodily posture. *Amer. J. Psychol.*, 1926, **37**, 261.

the subject reestablishes contact. Each loss of contact and each cycle are recorded on a paper tape polygraph.

What are the properties of this pursuit task which make it an especially good one for experiments of the learning type? Meyer[9] has pointed out that "all true learning (substituting a different reaction) consists then and thus, never in improving a substitution already forming, but first in breaking down the inherited reaction." The 'inherited' response to such a moving 'stimulus' is that of active, tense, optical-manual pursuit. Frequently the reactor makes erratic compensatory movements of the hand and body, discovers the task to be more difficult than it looked in advance, and tries to force or 'anticipate' the predetermined course of the instrument. Under the stress of such tension, performance grows less effective and to learn, the subject must reverse his whole general posture (*Einstellung*) and become a sort of passive, relaxed follower of the moving bead.

He also finds that cutaneous pressure and kinaesthesis are better 'controls' than foveal vision, and that tight gripping of the stylus only blunts these discriminations and brings on quickly the inaccuracy and unsteadiness incidental to wasted energy and 'fatigue.' Thus the pursuitmeter furnishes a learning task that involves something more than the mere fixation of a 'simple' coordinated movement.

Practically without exception the protocols of our subjects show that the first or first and second sittings begin with a set of self-imposed instructions that are as directly opposed to the best method as can be conceived. These of course have to be unlearned or abandoned before real progress can be made. They involve such things as: "hold the stylus near the hammer head," which of course makes it easier to break the horizontal contact; "use pressure on the stylus," which makes it slip off contact since both surfaces of stylus and bead in apposition are slightly rounded; "watch the bead," "try to guess where it is going next"; "get set for changes in rate or direction." These things all apply specifically to Groups I and II but not to Group III, whose instruction included warning against all of them.

The Experimental Groups and the Methods

After a very brief demonstration Group I was given only the instruction to keep the instrument running continuously. Beyond this the subjects[10] were left to their own devices and were not questioned or

[9]Meyer, M. F. The equation of the learning function. *Amer. J. Psychol.*, 1923, **34**, 203-222.

[10]The fifteen subjects were all students in the second quarter of the elementary course and none had ever participated in any similar experiment. Seven of the group were women.

asked to write out their protocols at the conclusion of the sittings, except as they reported as *Kundgaben*.

Group II received the additional instruction to use as much cleverness or analysis as possible in finding the most effective method of keeping the instrument going with the buzzer silent. Each S was read

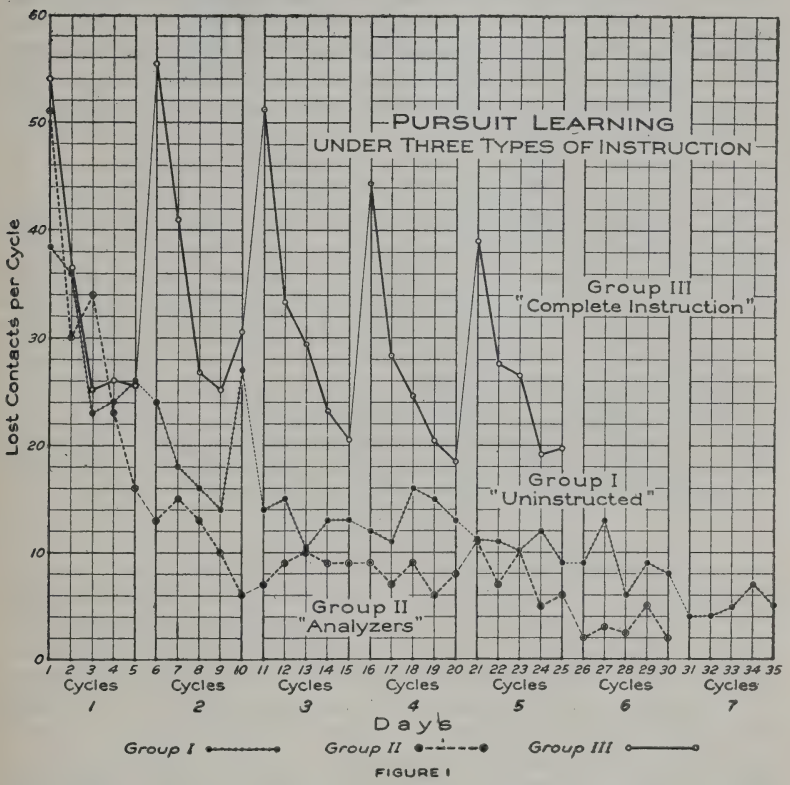

FIGURE 1

a carefully prepared sheet of instructions before each sitting, urging him to "try to remember the things you do which improve your method," to "put in writing at the conclusion of each sitting as carefully as you can just what you did during the sitting" and was also told "between this sitting and the next one (on the third day following), try to figure out for yourself the system that will get the best possible results." This group was designated the analytical group.

Group III received the simple demonstration of how the machine was operated. In addition at each sitting a set of instructions was read to these subjects which described in detail all the known facts

about the nature of the task, its difficulties, the best methods of operation, etc., that had been gained from a more extensive investigation with the instrument by one of us (R) with sixty subjects. Thus these subjects learned in a dozen specific sentences to tell *another* person exactly what to do to produce the best result prior to any specific 'practice.' Each sitting for each subject consisted of five three-minute cycles. Thus each subject ran the instrument a total of twenty-five cycles with at least a day's interim between the sittings. Each curve thus represents a total of 125 cycles for the five subjects of each group. No striking differences were observed between the individual subjects of each group at the first sittings.

Results and Conclusions

Figure 1 presents graphically the curves of the means of the three groups by cycles, and by days or sittings.

It is evident that the curves for Groups I and II show only slight differences in general form and limits throughout the entire course of the experiment. The small difference between the curves for these two groups points to the fact that although each received a different instruction, it was hardly possible for the 'uninstructed' group (Group I) to avoid assuming a general *Einstellung* similar or identical to that of Group II, the 'analyzers.' Both curves present the features of learning curves which are of the general form of an arc cotangent function, as described by Meyer [11] where

$$t = T + K \text{ arc cot } (n + p)$$ [12]

A fair approximation for the curve of Group II may be had by ascribing the following values to the constants: $p = 1.5$, $K = 1.3$, and $T = 0.5$. The curve of Group I may likewise be approximated by assigning the same values for p and T, but by making K slightly larger.

The curve of Group III illustrates clearly the marked difference in performance of this group from Groups I and II, *although the initial performance was practically identical with that of the most rapid learners,* the 'analyzers' of Group II. The outstanding features of the curve of Group III are the uniformity of its shape from day to day and its significant difference from that of the other two groups. There seems to be little doubt but that the effect of greater verbal

[11] *Loc. cit.*

[12] The definitions of the constants in this equation are as follows: n is the number of specific 'tests' or practices, repeated at regular intervals; t is the total duration of any test or practice; T is the limit of t, no matter how large n becomes; p is a parameter of the combined contribution of inheritance and previous training; K is a constant expressed as equal to $(L—1)\ T/\pi$; L is the original learning 'ability' and equals $1 + \pi K/T$.

For the derivation, mode of calculation, etc., the reader should consult Meyer's paper, cited in note 9.

instruction, where the *S* first *learns* the pursuit act linguistically, is to impede and retard the acquisition of manipulatory skill. This seems to be further emphasized by the fact that performance in the last three cycles of each sitting is distinctly superior to the first two. This may be taken to indicate that significant progress is made only after the *S* stops implicit speech directions and shifts to a method of sheer manipulation.

How much difference must there be in the form and limits of curves such as those of Fig. 1 in order to claim for them validity of the differences and to ascribe these differences to method rather than to sampling errors? The writers are inclined to answer this question in the way Dunlap[13] has answered it. Any statistics of variables is no better than the prime description of the variables, which is an experimental and not a statistical problem. Theoretically the artificiality of learning curves in general, plotted as they frequently are without regard to the fact that the curve may not be the curve of a continuous function at all, renders even the comparison of the harmonic means of the three curves a doubtful undertaking.

On this ground we have taken the view that our curves are gross measures of the performance, not only of whole organisms at work but in this case of a group of five organisms. This yields a mass of data which can perhaps best be treated at present by gross 'qualitative' description. More refined mathematical analysis must be left for subsequent development after we have more precise information regarding the specific variables involved.

If appeal is made to the alleged 'law' of the single variable or to the plan of selecting out of a larger number of subjects only those who make up 'identical' groups (so classified because of the identity of initial performances), then it is assumed that whatever differences in the ratios of the deratives of x and y (the rate of learning) appear must be due to some factor or factors, such for example as the constancy of the *Einstellung* imposed by the instruction.

This view seems to us to have in it two fundamental errors: (1) Perhaps no variable is truly independent or isolable, except as a pure speculation. (2) The shape of the curve is not in any sense a mathematical function of the value of y where n is small, i. e., at or near the beginning of practice. This the reader can verify for himself by an hour's work with Meyer's equation in fitting a few curves to empirical data. Since the value of the constant K has much greater influence on the form of the curve than the parameter p of inheritance and previous training (of whatever sort), we cannot admit that the task of equating groups in terms of initial performance is a safe method

[13]Dunlap, K. The methods of experimental psychology. In Psychologies of 1925, 331-351. Worcester, Mass., Clark University Press, 1926.

of insuring that only one factor is being varied in the experimental program.

We prefer to show that there are differences in the curves and to regard the *visible* differences in the forms of the curves (Group III as distinguished from Groups I and II) as the starting point of another problem.[14] We have met the demands of our problem if we can show that significant differences in the curves accompany significant differences in method. What the curve seems to represent optically may not conform at all to another type of description based on what the learner was actually doing in the time intervals represented as its abscissæ or what transpired in the interstices between the 'practices.'

Suppose we compare the three groups with respect to performance on the first and fifth days by listing the mean number of 'breaks' of the stylus-bead circuit. This is an indirect measure of the time required to run the instrument through one cycle. This should give some notion of the comparative initial performances and the effect of the practices under different instruction on the relative differences of the groups. Table I presents this data.

TABLE I

Group	Total 'Breaks' of Stylus-Bead Circuit	
	First Day	Fifth Day
I. Uninstructed	147.0	41.0
II. Analytical group	154.0	40.0
III. Fully Instructed group	166.0	132.0

The same essential differences in performance between Groups I and II in cycles 1 to 5 (first day) are maintained during cycles 21 to 25 (fifth day). However, while group III may be said to show only a slight difference in performance from Groups I and II for the first day (e. g., the difference between Groups I and III is but 19 'breaks' in 15 minutes or only about 1.26 'breaks' per minute), yet the striking difference in fifth day performance is of such magnitude that it is extremely unlikely that the difference may have been due to 'chance.' The descriptive trend of this curve indicates that for types of pursuit learning in which the pattern of the movements is not reducible to verbal representation, preliminary 'teaching' or instruction of the language type is detrimental.

In summary, *Einstellung* is controllable *only in part* by imposed linguistic instruction. Within limits and for certain specific tasks the verbal part of habits may help to foreshorten the learning series. Beyond these limits (at present undetermined) and for other types of skills it may actually impede or hinder progress. The general case

[14]For a similar view cf. Melrose, J. A. The method of organic problems, *Psychol. Rev.*, 1925, **32**, 74-82.

in which language inhibits is one in which substitutive or surrogatory verbal habits cannot be made effective substitutes for the direct sensory stimuli afforded by the task itself. The pursuitmeter, for example, can only be operated by the hands, not by the larynx.

The form of pursuitmeter used in this experiment offers to experimental psychologists an instrument 'sensitive' enough to record what Dunlap[15] has called psychological condition. Studies of this type, now in progress, will be reported later.

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[15]Dunlap, K. Apparatus and methods for measurement of psychological condition. *J. Comp. Psychol.*, 1926, **6**, 133-138.

NOTES

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TITCHENER FILM

Professor Titchener attended the informal conference of a group of experimental psychologists at Harvard University, Cambridge, Massachusetts, on April 18-20, 1927. At that time he seemed in very good health and, as usual at these conferences, took a leading part in most of the discussions. On the second day, April 19th, when the group had finished lunch at the Harvard Union and was strolling across the lawn between the President's House and the Widener Memorial Library, some motion pictures were taken. Professor Titchener had expressed interest in the camera as a mechanism and suggested to me that it be tried on the group, therefore, Professor Herbert S. Langfeld and I took turns operating it. The result is a strip of film (Eastman Ciné-Kodak, 16 mm.) not quite 50 feet long including title. In some parts the camera was moved a little too rapidly from side to side in an effort to cover the entire group, other than that the picture is satisfactory. Titchener is shown smoking and conversing with Dodge, Yerkes, and other American colleagues. The last section is taken on the West Steps at Emerson Hall.

Since the death of Professor Titchener a number of his friends and former students, who have seen the film, have requested copies. I shall be glad to supply a copy at cost (about $3.00) to anyone who may desire it and am keeping the original film as a master from which copies may be made in the future.

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GROUP DEMONSTRATION OF THE BLIND SPOT

No experiment is more impressive to the beginning student in Psychology and Physiology than to note the disappearance of a prominent black circle when it falls on the blind spot. A majority of our textbooks contain the well-known two-dot diagram with instructions for directing the individual student. But not a few fail to get just the right distance or some other of the simple conditions and so miss the thrill.

The writer has found it very practical to arrange a group demonstration of the blind spot which is carried out in the following manner. A white disc 6 inches in diameter is attached to the blackboard near the right end as viewed by the class, then starting 1 foot from the center of the disc and

proceeding from right to left near the upper edge of the board, one numbers off the feet with chalk, and so makes a scale extending about 20 feet from the disc. Half-way marks are placed between the numbers so that the distance can be easily gotten to the nearest half foot. Figure 1 makes this arrangement clear. Thus if I fixate the 7 on the scale I will be looking at a point 7 feet to the left of the disc, if I fix the mark between 7 and 8, the distance will be 7½ feet, providing I am standing in a position at right angles to this portion of the wall. The directions to the class are then as follows: "Hold your left hand over the left eye so you see only with the right eye. Look at the white disc and then look along the scale slowly proceeding from the nearest number to the next while you fixate the numbers in turn. The disc can be seen at the same time, but you will perhaps find quite a definite point where the disc disappears. Continue from this position on the scale progressing to the left and you will observe that at another quite definite point the disc reappears and continues to be visible while you fixate in turn several more positions farther at the left. In this experiment make several careful trials and so determine (1) that point on the scale where the disc first disappears for you and (2) the point at which it reappears."

Each student is provided with a mimeographed sheet on which is shown a diagram such as Figure 2. Suitable blank spaces are provided for the student to fill in the scale distance for the disappearance position and the reappearance position. The seat distance is marked on the chair which he occupies and he therefore can answer from data which he secures at the group demonstration, the following three questions: (1) What is the distance from the fovea to the nearest edge of the blind spot? (2) What is the distance from the fovea to the farthest edge of the blind spot? (3) What is the width of the blind spot?

Arrangements for this group demonstration may be of a temporary nature like this described above where the blackboard is used or (2) one can use a black disc on the white or gray wall above the blackboard and have a series of numbers to constitute the scale permanently painted on the wall, or (3) have a long chart made of a roll of sign painters cloth which can be hung up for the demonstration. I have used this latter, a chart some 40 feet long in a hall that would seat 1500 people and have gotten very satis-

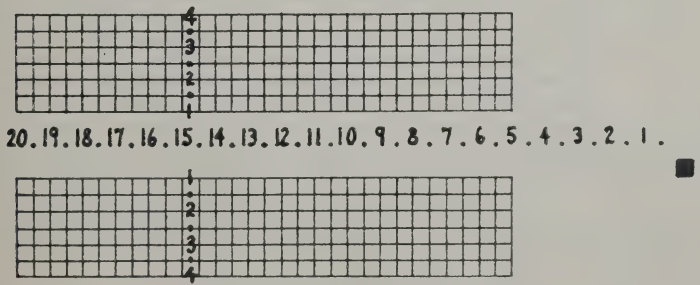

FIGURE 1

A Scale for Examining the Size of the Blind Spot in the Right Eye

The central horizontal portion represents the arrangement placed on the wall for group demonstration.

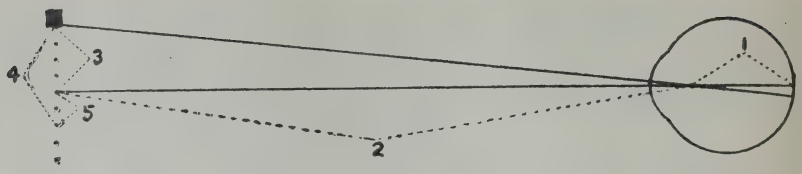

FIGURE 2

THE BLIND SPOT PROBLEM DIAGRAM

1. Distance of the second nodal point from the retina. The value of 0.6 inch or 15 mm. may be used.
2. Observer's distance from the wall
3. Disappearance distance
4. Reappearance distance
5. Blind spot diameter on wall

factory results with a class of 600 occupying the central portion of the room. For the ordinary lecture room a permanent scale painted on the wall, is to be recommended. One can have a black square at the left hand end of the scale thus making it convenient to demonstrate the blind spot in the left eye as well. With a permanent scale and a few marks of known distance above and below the scale students can roughly observe the vertical diameter of the blind spot. A series of cardboard charts about 3 feet square made of different colors and each with a white or black 6 inch disc placed at the center can be hung at the end of the scale to demonstrate the filling-in effect. Such charts in combination with the wall scale also make it relatively easy to demonstrate the color fields of the retina. It provides a means of measuring on just one axis but that suffices to show the elementary student that the retina is not uniformly sensitive to the different colors. Another point of usefulness for such a scale is that is provides fixation positions for securing after-images in the peripheral field.

This group method of working with the blind spot is much more like the original demonstration of Mariotte and, what is more important, it provides a simple means of securing actual measurement for one diameter of the area. It will be recalled that Mariotte[1] placed discs on the wall for observing this phenomena, "Two white paper discs are fixed on a dark ground, the right one being distinctly larger than the left to facilitate easy distinction and at a distance from each other of 2 feet, so that the right one is a little lower than the other. The lower spot is observed with the right eye (the left one being closed) and the observer slowly steps back some 9 or 10 feet while keeping his right eye firmly fixed on the left dial. In doing so, the right spot will disappear from the field of vision." In another place[2] this same method of carrying out the demonstration is given with the further statement: "At 9 feet the second paper disappears from the field of vision but other objects further removed, stand out clearly. More surprising yet is the fact, that the same is true for black discs on a white surface, so

[1]Mariotte, Acta Eruditorum, 1683, p. 67.

[2]Oeuvres de M. Mariotte, Leyde (Leiden), 1717, Vol. II, pp. 495-534. I am indebted to one of my graduate students, Mr. H. Beaumont, for looking up the early publications of M. Mariotte and others, in reference to the blind spot. We made use of Helmholtz's bibliography to this early literature, See Psychological Optics, Southall Translation, Optical Society of America, 1924, Vol. II, p. 44.

that the surface has the appearance of being perfectly white. Other subjects find different distances to have this effect, which seems due to a different location of the optic nerve." The method of stepping back from the pair of objects as outlined by Mariotte or of moving the book farther or nearer is the one generally used. Our method of using successive fixation points at different distances from the disc while the subject sits or stands still is an obvious alternate to the classical method, but if it has been used before it has at least never gained wide recognition. Valentin[3] and Helmholtz[4] give probably the fullest discussion of the blind spot and the former illustrates several different methods of demonstration and measurement.

In concluding this note I will give a sample set of results from a beginning class in psychology, meeting in a room 34x38 feet. The class faced the 34 foot wall, and the distance from the disc to each chair was known and marked on the chair back. The data is summarized in the table.

TABLE I

Blind Spot Results From a Group Demonstration With an Elementary Class

No. of Observers	Seat Distance Feet	Aver. Disappearance Distance Feet & Inches	Aver. Reappearance Distance Feet & Inches	Aver. Blind Spot Diameter Inches
22	13-20	4-0	5-4	.045
47	21-30	6-7	8-6	.048
54	31-45	8-5	12-10	.052

The retinal distances may be given in actual dimensions expressed as decimals of an inch or in degrees of visual angle. For some reason the usual run of elementary students seems to prefer the former. The average blind spot diameters for the three groups of subjects compare favorably with the supposedly quite exact data cited by Helmholtz (Vol. II, p. 29). As expected, the observers who are farther away get a greater diameter as the disc is for them more nearly a point. It must be noted that this method does not necessarily find the widest diameter. The demonstration should be thought of as a 15 minute excercise.

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ON PLOTTING ONE'S OWN BLIND SPOT

In elementary laboratory courses in psychology the plotting of the blind spot is often used as an experiment, but I believe we commonly have a pair of students work together. This is not only a waste of good time

[3]Valentin, Lehrbuch der Physiology der Menschen, 1848, Vol. II.

[4]Helmholtz, *ibid.*, Vol. II Physiology texts have in general copied Helmholtz's blind spot figure (Figure 1) which shows a white disc and cross on a black field, thus following Mariotte. The arrangement of dots in Sanford, A Course in Experimental Psychology, Boston, 1895, Part I, pp. 102-104, is a pleasing variation from the usual diagram. However, the best treatment and set of figures given in any psychology text with which I am familiar is that in Thorndike, Elements of Psychology, New York, 1905, pp. 177-179, Figures 62-67 inclusive.

but is a particularly tedious task for the person who has to hold, move the target, and mark positions. Helmholtz plotted *his own* blind spot, a fact which seems to resound down through the textbooks. To quote from the Southall Translation,[1] "Place the eye 8 or 12 inches above a sheet of white paper on which a little cross is marked to serve as a point of fixation of the eye. Take a white or at any rate a brightly colored pen, with some ink on its point, and move it over the paper into the projection of the blind spot until the point disappears. Then move it from this place, first in one direction, and then in another, toward the periphery of the spot where it again begins to be visible." A small dot is made at each position just as the pen point comes into view. The illustration given by Helmholtz as well as his statements, make it clear that this method used by a skilled observer will show the boundary of the blind spot with such detail as to make clear the beginnings of the large blood vessels. Helmholtz states further, "If a small black spot is made on the paper, and different points fixated one after another, the continuation of the vessels far into the field of the retina will be found to be blind spaces."

We propose this latter method as an alternate and more practical one, at least for elementary students, for plotting the main border of the blind area. I devised this before I had discovered the last quotation in Helmholtz. It is based on the method outlined in the proceeding note of using successive fixation points along a scale which stretches away from the disc or dot that is to fall into the blind spot area. Therefore, in order to plot one's own blind spot with accuracy, the following simple procedure may be followed. On a page of any ordinary cross-section paper, preferably white, make a black dot 3 mm. in diameter, close the left eye or place a blind-fold over it, take a one-foot rule in the hand, sit leaning well over the table, place one end of the rule on the paper and rest the chin on the other end holding the rule about vertical, thus providing a constant distance for the eye and a fairly steady head rest. (Use a ring stand for this purpose if one is conveniently at hand.) Now look along the horizontal line which stretches to the left of the black dot until the position on the cross-section paper is found which when fixated causes the black spot to disappear. Take a fountain pen, place the pen point at the position fixated and then fixating the *pen point itself* move the pen slowly toward the left or right until the black dot just appears. There make a small mark with the pen. In this way proceed along the horizontal and vertical lines of the ruled paper in this region making small dots at those positions of the fixated pen point where the 3 mm. dot just comes into view. The plotting should be done quite rapidly so that one does not tire sufficiently to begin moving the head. The usefulness of the cross-ruled paper is obvious in providing preliminary fixation positions and in measuring up the area included in the blind spot as plotted at this distance. The spot plotted in this way is no different in shape than when plotted after the method of Helmholtz, by fixating a cross and watching for a pen point to appear beyond the border of the blind spot; in which case the rest of the pen and the hand are distractions. The plot on the paper will be simply displaced by a distance equal to that between the blind spot and the fovea. This method has then three chief advantages: (1) it is more rapid and therefore less tiresome than when people work in pairs, (2) it saves the time of one person and allows them both to get quite accurate results in the same observation period, and (3) fixation may be more easily held on the moving pen point than on the stationary dot or cross.

[1]Helmholtz, Physiological Optics, the Optical Society of America, 1924, Vol. II, pp. 27-28.

There is a further advantage in having the plot on a page of the notebook in place of making it on the wall or blackboard.

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THE PEEP-HOLE METHOD FOR OBSERVING EYE MOVEMENTS IN READING

It is well known that the saccadic eye movements used in reading escaped observation until recent times. The opthalmologist Dr. Javal,[1] who seems to have been the first to make scientific note of them, counted the pauses by watching the eye in a mirror. He placed this reflection surface somewhat to the front and to the right of the person who was reading and then standing behind the reader looked over his shoulder at the mirror image of the reader's face. Essentially the same method was used by Landolt[2] in his study of reading French and German sentences and single words. This was in 1891. Seven years later Erdmann and Dodge[3] used the mirror method and the alternate one of viewing the eye through a small telescope. Ohrwall[4] found it possible to employ a microscope placed quite close to the subject's eye and focused on a small blood vessel which he watched intently during the process of counting the rapidly succeeding fixations. A small head gear has been devised and described by Freeman[5] for holding a suitable mirror in the proper position for the experimenter to observe from behind after the same manner as originally used by Javal.

At Stanford we have found a more simple method of direct observation than any of these which have been mentioned. It involves only the making of a hole about ¼ inch in diameter somewhere near the middle of the page of copy that is to be read. The experimenter holds the copy, seating himself on a stool in front of the chair to be occupied by the reader. He holds the page in front of his face with the print toward the reader and with the small peep-hole close to one of his own eyes. The advantages of this simple direct method appear to be the following: (1) the observer is as close as possible to the eyes of the reader, (2) the eyes are seen from directly in front, and the movements are thus apparently larger than when seen at an angle through a mirror, (3) the direction of the reader's line of regard can be fairly accurately judged when the observer is looking through from the center of that field which the reader is covering, and (4) with the face of the observer thus hidden the subject reads with less distraction. The matter of avoiding distraction in making such observation is particularly important in examining young children and those who seem to have faulty reading habits. This ready clinical method appears to reduce the distraction feature to a minimum. The eye move-

[1]Javal, E. Essai sur la Physiologie de la Lecture. *Ann. D'Oculistique*, 1878, **79**, 97-117, 240-274.

[2]Landolt. Nouvelles Recherches sur la Physiologie des Mouvements des Yeux. *Arch. D'Opth.*, 1891, **11**.

[3]Erdmann, B., and Dodge, R. Psychologische Untersuchungen über das Lesen auf experimenteller Grundlage, Halle, 1898.

[4]Ohrwall, H. Die Bewegungen des Auges während des Fixierens. *Skand. Arch. f. Physiol.*, 1912, **27**, 65-86, 304-314.

[5]Freeman, F. N. Experimental education. Boston, 1916, 95-139.

ments of skilled adult readers are hard to count by any direct method of observation but slow readers and children can be satisfactorily examined by this peep-hole method, particularly for preliminary study.

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THE HAND CENTRIFUGE COLOR-MIXER[1]

A small color-mixer operated by means of a round leather belt is usually a piece of apparatus out of order. A substitute piece can be readily supplied by using one of the small inexpensive hand centrifuges. The head carrying the support for the tubes is removed and on the main shaft in its place a disc clamp is fastened. The unit is shown in the accompanying illustration, (Fig. 3). Such mechanisms give a ratio of about 20 revolutions of the main shaft for one revolution of the crank. It is very easy to operate at a speed that provides even color mixing. As a device to use in lecturing to small classes it may be recommended for "the belt does not either come off or get broken at the critical moment." It can be held in hand and "pointed" directly at the students while the discs are being revolved so that all in turn may get a satisfactory view. It can be both started and stopped quickly. The head is made as two clamps (C and D) so that two pairs of discs differing in diameter may be revolved at once.

As a device to be used in the laboratory it has the following points to recommend it: (1) a clamp (B) secures it firmly to the table, (2) the plane of the paper discs is horizontal and therefore it secures good lighting and is rather more easily visible to the individual who is performing the color mixing experiment, (3) trials at matching can be carried out in more rapid succession than when using electrically driven color-mixers by reason of the fact that the centrifuge mixer can accelerate and decelerate more rapidly and can be started and stopped more easily than an electric motor.

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A ROTARY MOTILITY TEST

So many of the operations of industry and of every day life involve the rapid turning of a hand crank that this movement may well be made into a standard motility test. It need not be thought of as an alternate to tapping which in some form is the most common test of motor speed now in use. *Tapping* and *turning* are sufficiently unalike to not trespass on each other's "fields."

A suitable apparatus for the *turning test* may be easily provided and

[1]Such hand centrifuges can be obtained from any medical supply house and certain of these instruments imported from Europe are regularly on sale at prices rather less than ten dollars for the complete outfit. The author does not know of any clamping-head commercially made and sold separately but this can be simply produced by the university mechanic.

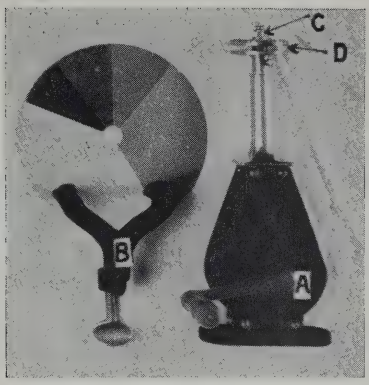

FIGURE 3

A Hand Centrifuge Converted Into a Color-Mixer

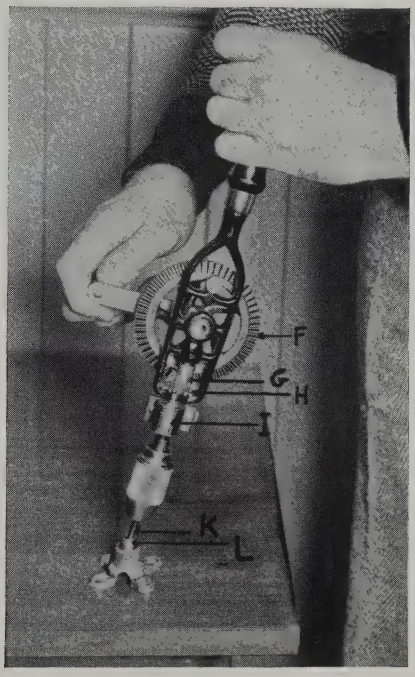

FIGURE 4

The Ordinary Machinist's Drill Arranged With a Counter to Register the Turning Speed of the Hand

at a relatively small expense due to the commercial development of the small hand drill. To be perfectly specific I will give details of the items which have been combined in our own test piece. A Millers Falls hand drill No. 1, which can be gotten at almost any hardware store, has been fitted with a Veeder counter No. 4D8 (revolution counter shaft on left, top of shaft turning from observer, 5 figure, price about $2.25, Veeder Manufacturing Co., Hartford, Conn.). A small 16 pitch gear with 14 teeth and a ¼ inch hole, No. G 230, Boston Gear Works, Inc., was used on the ⅛ inch shaft of the Veeder counter. A bushing is necessary to mount the gear on the counter as the gear can not be had with a ⅛ inch hole. The final arrangement is as shown in Figure 4. The handle, that is, the radius of the circle described by the hand is 2⅝ inches. The large gear (F) has 56 teeth; the small gear (G) mounted on the main drill shaft has 13 teeth; the gear (H) mounted on the Veeder counter and meshing with G has 14 teeth (all three gears have a 16 diameter pitch). The Veeder counter (I) is simply clamped to the drill frame just above the chuck. In place of the ordinary twist drill a small piece of drill rod (K), with a well-rounded end, is clamped in the chuck. This rests in a small metal cup (L) which may be of any convenient construction. It should have a diameter of about ¼ inch and a depth of ⅜ inch and contain a few drops of oil.

The *turning test* as we have used it has been for a 10 second period. The subject grasps the mechanism as shown in Figure 4. The hand crank is held straight up, the counter is read. It is then explained to the subject that the forearm should be in a position which is about perpendicular to the plane of the crank arm at the handle end. The signal "Ready, Go" is used to start the operation and the word "Stop" is spoken very sharply at the end of 10 seconds. A stop watch is used. The subject is cautioned to stop promptly at the signal and to hold on to the crank handle (see the illustration), that is, not to give the handle a final fling. A further caution is given the subject that he grip the rod rather tightly so his hand will not fly off and thus cause fumbling. The Veeder counter tallies 463 for each 100 circular movements of the hand.

Dr. Robert H. Seashore[1] has used the turning test as one member in a battery of motor tests. His data on this test show a reliability of .88. This correlation is based on results with 50 Stanford men, each man being given fifteen 10 second trials on each of two days. Dr. Seashore's data show what we may expect as about normal with this test. Taking the average of five 10 second trials per period on each man and ten men, the average results are the following:

	First Day			Second Day (48 hours later)		
Periods	1	2	3	1	2	3
Score on Veeder Count	242	254	256	260	265	267
Circular Revolutions of Hand	52.1	54.9	55.3	56.1	57.2	57.7

The average value of 57.7 as the circular revolution speed (10 second test) for normal young men who have had a fair amount of practise with the test, is not a great deal less than the number of simple finger movements

[1]National Research Fellow at Stanford.

that can be performed in the same interval. The average tapping or finger movement score for comparable groups of men is about 65.2.[2] Comparative results with the *turning test* with other motor performances will be published later by Dr. Seashore. As carried out with this modified hand drill, subjects are always well motivated for the test. Also it can be easily used in connection with studies of handedness.

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THE DISCRIMINATION OF MAJOR, MINOR, AND CERTAIN MISTUNED CHORDS

The early missionaries and explorers, in observing the musical modes employed by 'primitive' peoples, were often wont to claim the frequent use of the minor mode by these peoples. Although these claims have been substantiated to some extent by later ethnological investigations, many of the apparent examples of the minor mode have been found to be either mistuned majors (to occidental ears) or else entirely foreign modes. The present study has grown out of the attempt to measure the efficiency with which university students discriminate major, minor, and certain mistuned chords.

Among the battery of special ability tests devised by Max Meyer,[1] one finds the terpometer, the test used in the present study. The stimuli of this test consist of forty broken successive chords, twenty of which are mistuned, ten are majors, and ten minors. The test is given on a specially constructed Lyon and Healy reed organ which has two keyboards placed end to end. The left board is similar to the right except that each key (white or black) is tuned a quarter-tone lower than the corresponding key on the right bank. Thus the majors and minors can be given on either bank of keys, but the mistuned chords comprise tones of both banks. The tonal frequencies of the right bank ranged from 86.31 d.v. to 244.14 d.v. (even temperament).

The stimuli (varied a little from Meyer's procedure) consisted of ten broken major chords of the following sort—four of the root position, three of the first inversion, and three of the second inversion; ten broken minor chords and twenty mistuned chords of similar structure. The chords were always played in this order: low note, middle, high, middle, low, high, middle, high, low, middle, low, high, (as *c, e, g, e, c, g, e, g, c, e, c, g*).

The directions were those used by Meyer. "If the tones heard seem suitable for composing music of an active, vigorous character, write 'Active.' If for sad, melancholy music, write 'Sad.' If in doubt, or if there seems to be something indescribably wrong about the 'scale,' write 'Neutral.' (If you know the distinction 'major-minor,' remember it.)" Active, of course, meant major; sad, minor; and neutral, mistuned.

Before the test was given, the subjects were given a sample of each of the three types of chords on the three positions, making nine examples in all. For certain of the experiments, the mistuned chords were minors with the third note above the root dropped a quarter-tone (*c*—3 semitones—e^b, *g* is the minor and *c*—2½ semitones—e^b, *g* is mistuned). The third note was chosen to be the neutralized one since most scales approximate the tonic

[2]Miles, Alcohol and Human Efficiency, Carnegie Institution of Washington, Washington, D. C., Pub. 333, 1924, p. 244. See also in Pub. 280, p. 623.

[1]Meyer, Max F. Special Ability Tests as Used in Missouri, Including a Demonstration of a Typical Test. (Paper read at the American Psychological Association, December, 1923.) *Psychol. Bull.*, 1924, 21, p. 115.

(root) and the fifth above, but not necessarily the third. The stimuli were given in a random order, and the tones were played at the rate of 1 per second with 5 seconds interim between chords.

A typical discrimination table is as follows:

TABLE I

Actual	Called		
	Neutral	Active	Sad
Neutral	54%	14%	32%
Active	15%	57%	28%
Sad	21%	35%	44%
Frequency	30%	35%	35%

This table should read: "Of the neutrals, 54% were correctly named, 14% were called actives, and 32% sads, etc."

Although the data vary somewhat, a group of twenty-five university students will give about these results every time. The neutrals and actives are easier to discriminate than the sads. More neutrals are called sads than are called actives. More actives are called sads than are called neutrals. More sads are called actives than are called neutrals.

Reliabilities where the tests were given again a week later have been obtained from .658, *P. E.* of .086, to .747, *P. E.* of .058. There is little or no tendency for a higher score on a retest. One group made a mean score of 20.3 (out of 40) on the first test, and 21.4 on the second; another made 24.3 on the first test, and 23.9 on the second.

Hastening the speed of presentation to 1½ or slightly more tones per second decreases but slightly the votes for sad. This may be surprising as much more of the 'sadness' in minor music is associated with the time, than with the tonal structure. The increase in speed does facilitate the discrimination of neutral chords, however, from 5 to 15%; and usually the discrimination of actives and sads as well.

TABLE II

Actual	Called		
	Neutral	Active	Sad
Neutral	64%	11%	25%
Active	14%	64%	22%
Sad	16%	33%	51%
Frequency	31%	36%	33%

When the total stimuli are presented as simultaneous rather than as successive chords, the facilitation of all discriminations is even more marked.

TABLE III

Actual	Called		
	Neutral	Active	Sad
Neutral	81%	7%	12%
Active	3%	77%	20%
Sad	13%	27%	60%
Frequency	32%	37%	31%

It was thought that the mere fact of dropping the minor chord third a quarter-tone might have forced the neutral to be more often discriminated as sad than as active. To eliminate this I gave series of tests to different groups where the neutralized element was made the quarter-tone between the major and minor third (*c*—3½ semitones, *e*, *g*, as with the Siamese). As far as could be judged there was no difference resulting.

TABLE IV

Actual	Called Neutral	Called Active	Called Sad
Neutral	59%	13%	28%
Active	13%	62%	25%
Sad	25%	26%	49%
Frequency	32%	34%	34%

As might be expected, the individual discriminations varied considerably with musical training. That is, the students who had had formal music training made, on the average, more correct discriminations. There were numerous cases however, where subjects with considerable training made low scores, and where subjects with no formal musical training made extremely high scores. As Burks[2] in statistics and Brennan[3] in her work on music tests have pointed out, in correlation work one frequently does not know which element, if either, is the cause. In other words, do subjects who make high scores do so because of musical training, or do the high scorers gravitate toward music lessons? Be that as it may, a crude measure of the relationship is as follows. In one class, the mean score of the untrained was 17.8. The musically trained (who had any numbers of lessons) made 26.6. In another class the scores were 17.3 and 24.6 respectively.

In summarizing, at least the following points seem clear.

1. Broken mistuned chords (with the third of the minor triad a quarter-tone low or high) are more often discriminated as minor than as major chords. This may be a factor in the early missionary reports which claimed a surprising frequency of minor melodies among 'primitive' peoples.
2. Mistuned and major chords are easier to discriminate than minor chords. Both 1 and 2 may perhaps be explained on a 'conventionality' basis. We hear the major chords most frequently. Thus, if a new chord is presented, it is less conventional and so more likely to be called minor. The major chord would be easy to identify because of its commonness, the mistuned because it is so obviously 'off.'
3. An increase in the time rate of tonal presentations increases the accuracy of discrimination. It slightly decreases the frequency of votes for minor. This increase in time keeps adaptation from setting in and thus preserves the differences in chord character.
4. Where the chords are played as simultaneous rather than as successive sequences, the discriminations are facilitated even more than with increases in the speed of successive presentations. This is probably due to the added cues, such as roughness, beats, etc.
5. Retest scores are, on the average, but little greater than the scores on the first test.
6. Retest reliabilities have been obtained from .658, *P. E.* of .086, to .747, *P. E.* of .058.
7. In general, subjects with musical training score significantly higher than subjects without musical training.

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[2]Burke, B. S. On the Inadequacy of the Partial and Multiple Correlation Technique. *J. Educ. Psychol.*, 1926, 17, 532-540.
[3]Brennan, F. M. *University of Iowa Studies*, 9, 1926.

MONOGAMY AS A GENETIC FACTOR

It is easily understood, but not always adequately recognized, that considerably different genetic phenomena may be expected to occur in a population where monogamy is the rule from those observable where it is not; for, in the first place, sterility in the mate (under strict monogamy) cuts off the individual's germ plasm from reproduction as effectively as if he himself were sterile; and in the second, matings with more than one partner increase proportionately (even if the marital coefficient be identical) the number of possible recombinations, and, therefore, presumably the variability of offspring. Furthermore, celibacy (which is to be regarded as a minus deviation from pure monogamy) must be recognized as a marked genetic influence.

One of the chief obstacles to the determination of the effect of this factor on the quality of populations is the difficulty of measuring it satisfactorily. I make no claims to having solved this somewhat vexatious problem, but for the sake of facilitating discussion I suggest, as a first approximation to a suitable measure, the number of matings (defined in accord with whatever purpose the investigator has in mind) divided by half the population. This has the advantage that the perfectly monogamic population has an index of 1.00, from which minus deviations indicate celibacy and plus deviations polygamy. Some of its disadvantages are (1) that the greater the "monogamy index" (above 1.00) the less the monogamy, (2) that it fails to differentiate between celibacy and polygamy in populations in which both occur, (3) that it does not take into account types of polygamy or sex ratios (e. g., in a population of one man and three women but one type is possible, whereas in a population of two men and two women both men may marry both women or either may marry both while the other remains monogamous) and (4) its relations to the marital coefficient are completely unknown. The latter point is particularly intriguing, since the writer and a collaborator (Neglected Factors in the Differential Birth-Rate Problem, *Ped. Sem.*, 1927, **34**, 373-393), among others, have shown that it is reasonable to suppose the marital coefficient to have important genetic effects. In a purely monogamous population or one with celibacy as the only deviation therefrom, the marital coefficient is readily defined and easily ascertained; but with the introduction of polygamy it takes on the elusiveness common to the other family "constants," and for the same reason—viz., that the populations are no longer clear-cut; when *A* marries *b* and also *c*, he appears in the correlation twice, a contingency which the ordinary correlation concepts were not designed to meet. It may turn out, of course, that *A*'s genetic influence is not correctly weighted unless he *does* appear twice; but such a conclusion can be reached only after a careful consideration of the logical aspects of the problem, and cannot be answered *a priori*.

In the hope of drawing attention to this interesting factor in the genetics of populations (which is not without its practical aspects, inasmuch as actual deviations from perfect reproductive monogamy are probably of sufficient magnitude to influence the composition of real populations) I have made up some miniature populations and "bred" them through one cycle by the method of artificial populations previously described[1] (*op. cit.*).

[1]The method is briefly as follows: Given parents (e. g.) of genetic constitutions *AA Bb Cc dd Ee* (=*dhhrh*) and *Aa BB CC dd ee* (=*hddrr*), any offspring must be *A B C dd e*, plus chance arrangements of the heterozygous genes. Determining these by dice throws, three children of these parents may be *hhhrr*, *ddhrh*, and *hdhrr*. Assuming a trait to

These populations are equated as to number of individuals in the parent (P_1) (20) and the daughter (F_1) (30) generations; and they are differentiated as to the "monogamy indices." Or, more accurately, the same P_1 population is bred three times with three different indices, viz., 0.8, 1.0, and 1.9. The selection of the P_1 individuals was from the data of the preceding experiment, except that the same genetic constitution was not repeated; as a matter of chance, the extremes are rather more heavily represented than in the previous work or the general population (σ=29, as against about 20 for the former experiment), though the mean is practically coincident (M=98.6). The mating in all three cases is at random (psychologically, as was the selection) and the assignment of children at random in the sense that they were assigned to matings in as nearly equal proportion as possible in order from the first of the list. Thus, although the above remarks on the elusiveness of the marital coefficient apply and neither the marital nor the fecundity-intelligence coefficient is known, it is likely that they are approximately zero, and therefore equal, in all three cases. Since the populations are small, the data may be given in full:

P_1 Generation

A, *hrdhd*,	130	*a*, *rdhdd*,	76
B, *hrdrd*,	146	*b*, *rdhhd*,	80
C, *rdrdr*,	30	*c*, *rhrrd*,	82
D, *rhrdh*,	54	*d*, *rrhrd*,	126
E, *hhhdd*,	98	*e*, *hhhhd*,	102
F, *drdhd*,	130	*f*, *rhhhh*,	76
G, *rrhdd*,	104	*g*, *hrhrd*,	146
H, *rhrhd*,	64	*h*, *rrrhd*,	90
I, *rrrdd*,	86	*i*, *rrddd*,	106
J, *rrhhd*,	108	*j*, *hrhhd*,	128

0.8 Series

A x *a* =	*hhhdd*,	98;	*rhddd*,	80;	*hhddd*,	100.	
A x *b* =	*hhhhd*,	102;	*hhdhd*,	104;	*rhdhd*,	84;	*hhhhd*, 102.
D x *c* =	*rrrhd*,	110;	*rhrhh*,	58;	*rhrhd*,	64.	
D x *d* =	*rhhhh*,	76;	*rhhhh*,	76;	*rrhhh*,	102;	*rrhhh*, 102.
E x *e* =	*hhhhd*,	102;	*rhhdd*,	76;	*hhhhd*,	102;	*rddhd*, 82.
F x *f* =	*hhhhh*,	96;	*hhhhh*,	96;	*hhdrh*,	116;	*hrdhh*, 124.
H x *h* =	*rhrrd*,	82;	*rrrhd*,	110;	*rhrdd*,	60;	*rhrdd*, 60.
J x *j* =	*rrhrd*,	126;	*hrrhd*,	110;	*hrhhd*,	128;	*rrddd*, 106.

1.0 Series

A x *a* =	*rhdhd*,	84;	*rhdhd*,	84;	*hhhhd*,	102;	
B x *b* =	*hhhhd*,	102;	*rhdhd*,	84;	*hhhrd*,	120.	
C x *c* =	*rdrhh*,	56;	*rdrhh*,	56;	*rdrhh*,	56.	
D x *d* =	*rrrhd*,	110;	*rhrhd*,	64;	*rrrhd*,	110.	
E x *e* =	*hhdhd*,	104;	*rdddd*,	78;	*rhddd*,	80.	
F x *f* =	*hrhrd*,	146;	*hhhhd*,	102;	*hrhrd*,	146.	
G x *g* =	*rrdhd*,	110;	*rrdhd*,	110;	*rrhhd*,	109.	
H x *h* =	*rrrrd*,	108;	*rrrhd*,	110;	*rhrdd*,	60.	
I x *i* =	*rrhdd*,	104;	*rrhdd*,	104;	*rrhdd*,	104.	
J x *j* =	*rrdhd*,	110;	*rrhdd*,	104;	*rrdhd*,	110.	

be multiply determined, the assignment of appropriate values to the gene phases permits a study of the variations of population constants under specified conditions.

1.9 SERIES

A x *a* =	*hhhhd*,	102.		
A x *b* =	*hhdhd*,	104;	*hhhhd*,	102.
B x *b* =	*hhdrd*,	102.		
C x *b* =	*rdhdh*,	70;	*rdrhh*,	56.
C x *c* =	*rdrhh*,	56.		
D x *c* =	*rdrhd*,	62;	*rhrhh*,	58.
D x *d* =	*rrhhh*,	102;	*rrrhh*,	84.
D x *e* =	*rhhhd*,	82;	*hhrdd*,	80.
E x *e* =	*hhhhd*,	102;	*drhhd*,	128.
F x *e* =	*hrhhd*,	128;	*drhrd*,	146.
F x *f* =	*hrdhd*,	130.		
G x *f* =	*rhhdh*,	72;	*rhdhd*,	84.
G x *g* =	*rrdhd*,	110.		
G x *h* =	*rrhdd*,	104;	*rrhhd*,	108.
H x *h* =	*rrrrd*,	108.		
I x *h* =	*rrrhd*,	110;	*rrrhd*,	110.
I x *i* =	*rrhdd*,	104.		
I x *j* =	*hrrdd*,	106;	*hrhdd*,	124.
J x *j* =	*hrddd*,	126.		

The means (ungrouped) and standard deviations of the three F_1 generations are as follows:

	0.8 *series*	1.0 *series*	1.9 *series*
M	95.1	97.5	98.7
σ	19.8	23.4	23.2

It is of course clear that the differences are considerably smaller than their probable errors ($P.E._d = 3.8$ in the case of the 3.6 difference between the greatest and least mean). There is, however, another way to consider their validity: assuming independence, the chance that $M_{1.0}$ will exceed $M_{0.8}$ is $\frac{1}{2}$, and the chance that $M_{1.9}$ will exceed both is $\frac{1}{3}$; the appropriateness of taking the resulting 5.1 as a measure of reasonable belief that the ascending order is not an accident is somewhat vitiated by the philosophic implications of the fact that the chance for any other order is identical; but the suggestive fact of an ascending order paralleling an ascending order may perhaps furnish some slight ground for opinion. My object, however, is not to present adequately verified findings, but to suggest questions; the contribution of this inquiry may therefore be summarized thus: Do increases in the monogamy index tend to augment the population mean?

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THE EFFICIENCY OF MUSCLE

We read on pp. 306-307 of P. H. Mitchell's *Textbook of General Physiology* that

> "If the muscle is made to give a series of single contractions, with different weights attached to it in each case, the gram-millimeters of work done show a maximum at a certain optimum load. Any load, smaller or larger than this optimum, will result in the performance of a smaller amount of external work. If the work done in such a series of contractions is plotted as ordinates against the respective loads as

abscissas, a curve such as is shown in Figure 77 [parabola] is obtained. Muscle, like man-made machinery, does its most efficient work when carrying a load that is neither too large nor too small."

All of this is *literally* true, but nevertheless somewhat misleading; it seems to contain no physiological information of importance, but to be equivalent merely to the statement that when a word (such as "work") has been invested with certain connotations by the process of definition, it will be found later to exhibit those connotations with which it has been invested. Saying nothing more than this is sterile, and saying anything more verges toward the fallacy of the sliding middle; as in the final sentence quoted, where "work" as the product of load by excursion has been metamorphosed to signify something like "effort" or "energy expenditure"; the same criticisms apply to "optimum"—it being psychologically difficult to keep in mind that a word which ordinarily means "best" has here been restricted by definition to mean only "corresponding to a maximum on a certain type of graph."

The evidence for this standpoint is simple, and bears on this proposition: Given excursions decreasing at zero acceleration with increase in load, it follows as a consequence of the definition of work (excursion multiplied by load) that the work-vs.-load curve will be a parabola convex upward, and will therefore have an "optimum"—more prosaically known as a peak. Letting E = excursion, L = load, W = work, and A = angle of descent of the *E-vs.-L* "curve" (which is by hypothesis a straight line), we have

$$E = -L \tan A, \text{ and therefore}$$
$$W = -L^2 \tan A$$

which is a parabola convex upward, and must necessarily be so for every pair of series for which the original conditions are fulfilled. (The constants of translation have of course been omitted here, but are not essential to the argument and can be readily supplied.) The location of the peak is easily ascertained by setting the derivative equal to zero, which shows it to be a function of A and the translation constant appropriate to L (the latter being merely a question of where the readings are begun). In the case of series which ascend and descend like L and E, but whose product is a constant, it may be shown that A is variable, that is, that the acceleration in the *E-vs.-L* curve is not zero; hence the case does not belong in the class treated here.

In short, the introduction of the work concept into this problem seems to be entirely superfluous and unwarranted by the nature of the material studied, and to make for confusion and false conclusions. The truly physiological problem would appear to be the adequate description of the relationships between E and L, particularly the value of the acceleration, and if this is zero, the magnitude of the angle A. The facts that, after this is effected, the series obtained may be further multiplied, and that the products so obtained have names and show graphic forms determined by such multiplications, are evidently quite extra-physiological.

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HOW SHALL WE USE THE TERM "MIND"?

This note is inspired by a recent article by Hollingworth[1] in which he defines mind in terms of redintegrative activities. He says (p. 89):

"Mind is but a name we give to natural sequences of this redintegrative type . . . .

"These redintegrative activities of the soldier are what we mean by his mind . . . The mind of the soldier is this repertoire of responses to symbols of past or absent contexts."

For the purposes of this note there is nothing particularly novel about this definition. It serves simply to raise the question as to whether we should use the term "mind" in the sense of activities or in some other way—or perhaps not at all.

For the "not at all" answer there is a great deal to be said. So much error and contradiction are to be found in the use of the word that perhaps it had best be abandoned altogether. But words are not easily abandoned. They are likely to persist in literature and in current speech long after resolutions have pronounced a death sentence upon them. Particularly is this true if the word is used to refer to some object, action, or quality to which reference is constantly needed.

In general, there are two sharply contrasting uses of the term "mind." It is employed to designate, on the one hand, mental activities, and, on the other, the thing which engages in these activities.[2] Hollingworth's "repertoire of responses" is clearly an example of the first. Titchener takes a similar point of view.[3] Among professional psychologists, McDougall may be cited as one who holds to the opposite usage.[4]

Though McDougall's discussion does not seem to the writer to be entirely satisfactory, it is the purpose of this note to support his side of the argument. The word "mind" should be used—if at all—to refer to the organic basis of mental activity rather than to the activities themselves. The reasons are two:

(1) Apparently there is a need for some inclusive term by which to refer collectively to the mechanism that is active when one sees, thinks, feels, decides, and the like. Some word appears to be needed for reference to the basis of mental activity just as the word "body" is used with regard to physical activity. It is possible, of course, to refer to the individual himself as the agent of mental activity—that is, *Jones* thinks, not his mind. But it is more or less useful to have names referring to one and the same organism as it acts in different capacities. Jones is farmer, citizen, and husband, and it is entirely appropriate to use any one of these terms to refer to him. Nor does anyone suppose that farmer, citizen, and husband are separable parts of Jones. In precisely the same way it is appropriate to speak of body and mind in referring to Jones. One or the other of these terms may be applied to him when we are emphasizing different aspects of his behavior. No idea whatever of the old philosophical dualism need be implied.

(2) The use of "mind" to refer to the organic basis of mental activtiy rather than to the activity itself has the advantage of approximating the the popular conception. Although Titchener, in an attempt to get away from the "insubstantial mannikin" idea of mind, applies the term to activ-

[1]Hollingworth, H. L. General Laws of Redintegration. *J. Gen. Psychol.*, 1928, **1**, 79-90.

[2]I am aware of course that thoughts, ideas, images, and the like have not always been considered activities in the sense in which the word is here used.

[3]See for example his *"A Beginner's Psychology,"* 1915, pp. 5ff.

[4]See for example his *"Outlines of Psychology,"* 1923, p. 35.

ities, he recognizes that this is not the common-sense notion. He says, for example:[5]

"It is not easy to decide whether mind is activity, or whether these various activities are activities of mind. On the whole, common sense leans to the latter view: the activities are manifestations of mind."

But why should we abandon attempts to describe the thing that thinks, even to the extent of taking away the term that by long tradition is used to refer to it? It would seem a much more direct and rational procedure to let the common man use "mind" as he has used it, to refer to the thing that engages in mental activity, and then to set about to correct his ideas concerning the thing about which he is talking. There is, indeed, a danger that the "repertoire of responses," of the opposite usage, will itself come to imply an entity that does not exist in fact, that the activities themselves will be thought of as constituting some agency of action apart from the individual as a biological organism.[6]

In brief, it is the suggestion of this note that either we abandon the term "mind" or else we use it in its common-sense meaning, brought down to date—the collective organic basis of mental activity. Let us cease to distort language to serve a purpose that can be achieved more simply and more directly.

H. T. Manuel

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NINTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

The Ninth International Congress of Psychology will be held at Yale University in New Haven, Connecticut, U. S. A., probably in August or September, 1929.

The officers of the Congress are as follows: President, J. McKeen Cattell of New York; Vice-president, James R. Angell of Yale University; Secretary, Edwin G. Boring of Harvard University; Treasurer, R. S. Woodworth of Columbia University; Foreign Secretary, Herbert S. Langfeld of Princeton University; Executive Secretary, Walter S. Hunter of Clark University; Chairman of the Program Committee, Raymond Dodge of Yale University; Chairman of the Committee on Arrangements, R. P. Angier of Yale University.

Besides these men the National Committee includes J. E. Anderson, University of Minnesota; Madison Bentley, University of Illinois; E. A. Bott, University of Toronto; H. A. Carr, University of Chicago; Knight Dunlap, Johns Hopkins University; S. W. Fernberger, University of Pennsylvania; William McDougall, Duke University; W. B. Pillsbury, University of Michigan; C. E. Seashore, University of Iowa; L. M. Terman, Stanford University; E. L. Thorndike, Columbia University; H. C. Warren, Princeton University; M. F. Washburn, Vassar College; R. M. Yerkes, Yale University.

This is the first meeting of the Congress in America. The previous meetings have been held as follows: Paris, 1889; London, 1892; Munich, 1896; Paris, 1900; Rome, 1905; Geneva, 1909; Oxford, 1923; Groningen, 1926. It is expected that the Congress in the United States will be truly international

[5] *Loc. cit.*, p. 6.

[6] Inasmuch as some feel obliged to pigeonhole everything in ancient categories, I wish specifically to deny that I am suggesting a "materialistic" explanation. Why preserve this dualism?

in character. The Americans hope that the appointment of some foreigners for lecturers and lectureships can be arranged near the time of the Congress, so that foreign attendance can be increased and international solidarity within psychology furthered still more. Most appointments of this kind at American universities would have to apply only to psychologists who speak English.

THIRD ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION AT THE UNIVERSITY OF WISCONSIN, MADISON

FRIDAY AND SATURDAY, MAY 11 AND 12, 1928

1. Friday Afternoon, May 11
   Professor C. L. Hull, University of Wisconsin, Presiding
   A. The Problem of Insight
   B. Experimental Papers, Program A

2. Friday Evening, May 11
   Professor J. R. Kantor, University of Indiana, Presiding
   A. Experimental Social Psychology
   B. Round-Table on the Problem of the Elementary Course, Professor M. F. Meyer

3. Saturday Morning, May 12
   Professor Harvey A. Carr, University of Chicago, Presiding
   A. A Symposium on Learning
   B. Experimental Papers, Program B

4. Saturday Afternoon, May 12
   Professor A. R. Gilliland, Northwestern University, Presiding
   A. Reports from the Laboratories
   B. Informal Conferences
   C. Business Meeting

5. Saturday Evening, May 12
   Annual Dinner and President's Address
   Professor A. R. Gilliland, Northwestern University, President

BOOKS

EDITED BY

KARL BÜHLER
W. J. CROZIER
JAMES DREVER
JOHN PAUL NAFE
A. L. SCHNIERMANN
RAYMOND H. WHEELER

Rosanoff, A. J. [Ed.] *Manual of Psychiatry.* (6th Ed. Rev.) New York: Wiley; London: Chapman & Hall. 1927. Pp. xvi + 697. $6.00.

This manual, which originally appeared in 1905 as a translation of de Fursac's "Manuel de Psychiatrie," has been altered and amplified in successive editions. The present edition shows little similarity to the first. De Fursac appears as a collaborator along with Hollingworth, Jarrett, Neyman, and Williams; but the major contribution is by Rosanoff himself.

The chief additions in the present volume are, according to the editor's preface: "Chapters or sections dealing with the following subjects: disturbances of nutrition in relation to mental disorders; psychoses associated with lethargic encephalitis; psychoses associated with pellagra; residuals of cerebrospinal meningitis; theory of personality; rest and diet in the treatment of mental disorders; parole system; prevention by eugenic measures; mental disorders of childhood and mental hygiene of childhood; military psychiatry; a state mental hygiene program; group tests of intelligence; educational achievement tests; guide to study of personality; and tables of intelligence quotient values. Also a number of illustrations have been added. The rest of the book has been extensively rewritten and enlarged. A glossary of technical terms has been embodied in the index."

The arrangement of the material is a distinct improvement over the last edition. The book now consists of five parts. Part I, General Psychiatry, includes general etiology and general symptomatology. Part II, Special Psychiatry, includes constitutional disorders, alcoholic disorders, drug addictions, syphilitic disorders, traumatic disorders, miscellaneous groups, and a chapter on personality. Part III, The Practice of Psychiatry, includes in addition to the usual diagnostic and therapeutic procedures, psychoanalysis, mental hygiene, and the application of psychology and sociology in psychiatry. Part IV, Special Diagnostic Procedures, includes among other mental and physical tests, complete directions for the Stanford-Binet Scale and the Kent-Rosanoff Association test. Part V, Appendices, includes the classification of mental diseases adopted by the American Psychiatric Association, tables of height and weight, and tables of intelligence quotients. The book ends with a combined glossary of technical terms and index of subjects and authors.

The point of view represented in this manual is indicated by the following quotation from the preface: "While the more important current theoretical conceptions of the various schools of psychiatry have been given a place in it, the main endeavor has been to present a description of the material of the psychiatric clinic, and methods of dealing with it—that is to say, technique of diagnosis, prevention, and treatment." The reviewer feels that the eclectic position thus adopted is highly commendable in the

present controversial state of the mental sciences and he regrets that it was not maintained in the exposition of psychoanalysis. This is thoroughly Freudian. The important contributions to psychoanalysis of Adler, Jung, Rivers, White, and others receive no mention.

An interesting and valuable aspect of this work is the sociological approach. Man is recognized as predominantly a social being; and it is emphasized that "Most psychiatric cases are brought to light as such through social maladjustment." There are chapters on the application of sociology in psychiatry, on medico-legal questions, and on mental disorders of childhood. Mental hygiene is discussed with reference to child guidance and training, and with reference to community and state mental health programs. In this connection it is recognized that the prevention of mental disease is in part a matter of dollars and cents. *"Mental health is purchasable."*

The chapter on "A Theory of Personality" is an interesting contribution to the psychology of this perplexing subject. One may wonder why it is included in a book on psychiatry. But the author approaches the question from the psychiatric standpoint. Abnormal types of personality are regarded as quantitative variations of normal types. The abnormal tendencies are merely inhibited in the course of development. A study of the one therefore throws light upon the other. Some interesting suggestions are also given as to the inheritance of personality types and on the diagnosis and analysis of personality. Rosanoff's views on personality are, however, already well known to psychologists. They were presented at first in the *Psychological Bulletin* in 1920.

The reviewer feels that the discussion of the psychoneuroses is perhaps the weakest part of the whole work, and that the views on hysteria are particularly open to criticism. They are essentially those of the traditional family physician who took an ethical rather than a scientific attitude towards his patient. The essential factor in hysteria is described as the presence of *"a concealed, illicit, ethically untenable motive,"* and hysteria and malingering are regarded as one and the same thing. The reviewer finds himself in disagreement with these views. It is surely not a function of the psychiatrist to concern himself with the ethical character of his patient's motives, but rather with their psychological nature and origin, and the possible presence of conflicting motives. It is surely also important to differentiate between the conscious or unconscious quality of the motivation. In discussing this distinction, the author says: "It is strange that so futile a consideration, one so obviously belonging to the domain of metaphysics and not science, as the question of degree of consciousness of a mental process, should become the preoccupation of scientific men and should be chosen as a criterion of clinical diagnosis!" This is a rather surprising statement in view of the fact that the author is not a behaviorist. It seems also scarcely compatible with the treatment of the subject of consciousness elsewhere in the book, e. g., in the general symptomatology, and in the discussion of catatonic stupor. Whether a metaphysical consideration or not, it is one that all psychiatrists concern themselves with on occasion, and one that in many cases and particularly in hysteria is far from futile. It is surely important even from "a medical viewpoint" to distinguish between the person who consciously simulates a symptom, such as paralysis, in order to avoid a disagreeable task; and a person who develops a similar symptom with perhaps the same motive, but who is not conscious of this motive at all or, if conscious of it in a general way, is at least not conscious of its relation to the symptom. It is also important to know not whether the motive is ethically untenable (which motives are?), but whether or not it is in conflict with the conscious ideals of the patient. After thus account-

Jour. of Gen. Psych., April, 1928. Vol. I, #2

ing for hysteria, the author omits discussion of the nature of the other psychoneuroses.

There are some other minor points which might be criticized such as the over-emphasis on heredity as an etiological factor in a large group of diseases which he classifies under the caption "constitutional disorders." One wishes that the author had adopted the classification of mental diseases proposed by the American Psychiatric Association. On the whole, however, the book is a well-written, comprehensive and up-to-date treatise on psychiatry and one which will be indispensable to the practising psychiatrist, and a valuable source of information for the student of abnormal psychology.

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Kelley, T. L. *Interpretation of Educational Measurements.* Yonkers: World Book, 1927. Pp. xiii + 363. $2.20.

Kelley has here, as before, done something that manifestly needed doing; and, also as before, one is a little put to it to know just what the significance is of the thing he has done. Chapter I is the ubiquitous "historical survey" which according to the fashion of the age must bob up with comical irrelevance in articles on polar exploration and pip in chickens alike; Kelley has gotten over this initial ceremony with relative painlessness, due to his unfailing good humor and sense of values—I could only suggest that the "Shibboleth" episode and the coeval alternative-answer test of drinking from one's hand as against face downward are even earlier examples of the performance scale than the poetry ability of the Syracusan captives. Still, the book is not a historical summary. Neither is it a primary manual of statistics, though the 47 pages of Chapter VII are one of the most serviceable digests I have seen. Nor yet a Guide to the Distracted Tester, though there is nothing from page 214 on but detailed analyses, largely in the form of judgments (by Kelley, Franzen, Freeman, McCall, Otis, Trabue, and Van Wagenen—Thurstone and Monroe having expressed inability) of tests upon tests—some 70 groups of them, with from one to a dozen in a group. I have seen doubts expressed as to the advisability of this sort of thing; but it seems to me all to the good. If there are 500 tests now on the market, any device which will promote the throwing away of 450 of them is desirable, even if one or two good ones may be lost in the process; if you don't believe this, try to find a test that doesn't violate most of the dictates of common sense, child psychology and mathematics, *and* that will do roughly what you want to have it do, instead of the things you find in statistics textbook problems.

It might perhaps be said that Kelley's main idea was the individual difference problem and its corollary the probable error; that is, its corollary when individual differences are thought of as Kelley thinks of them—viz., as geography achievement minus arithmetic achievement, etc. The latter is, of course, too bad; and he is too sensitive and humane a man not to be rather more than dimly aware of it. But at the same time, he is too much a mathematician to forego the alluring equations which would be true if (or are true to the extent that, which is negligible) these things *were* individual differences in any important sense. Here, of course, we get near the roots of the whole mental test movement—as the testers implicitly recognize by hastily steering the argument back from anything in the neighborhood of "Why do this?" to a consideration of the much easier question "How do this?" The former is said to be "philosophy," and so

out of the domain of science; but it is forgotten that all this exists in the name of education, and that education is in the nature of things about nine-tenths philosophy and one-tenth science. Kelley says that other more important individual differences do exist, but we must be content with school abilities because we do not as yet know anything about such matters as extroversion and personality. It is possible—in the same sense in which we do not know anything about internal medicine; but it is important to remember that when we start with very little ("for lack of anything more") and make a great fuss over it, no matter how accurate and involved the fuss, we still have in the end very little. So here: The difference between "arithmetic ability" and "language ability" may on occasion be important, even as measured by tests; but it has exceedingly little bearing on children or education; the primary questions in this latter all-but-forgotten field are of the order "Is the child happy?" and the fact that these cannot be answered by any objective test whatever will not excuse us from acting on their answers, secured in any way possible—unless, as seems not infrequently the natural inference, our educators wish to regard themselves as commissioned by the community to prevent the normal development of the young. The following from Susan B. Isaacs' "The Function of the School for the Young Child" expresses the point so aptly that I cannot forbear quoting it:

> "If we foist them (subjects) on children when they have no internal motive for mastering them, we have, of course, to give our attention to the minute perfection of the technique for learning and teaching them."

From Kelley's realization of this (of a clarity difficult to estimate) evidently comes his strictures of last year[1] on the educational system and his present insistence on the probable error and reliability; for tests in arithmetic will be used by teachers, after all, to determine a child's social adjustments, etc.; and bad as the system is, it is no doubt of advantage that they be good tests and that their error of measurement be clearly recognized by everybody concerned. Hence the conclusion that a minimum reliability of .50 is necessary for group diagnosis and one of .96 for individual diagnosis; evidence for this and other conclusions announced in the text is presented in a separate chapter (VIII), thereby adding the functions of a research monograph to the numerous characters of the book. Among these other conclusions a prominent one is that 90% of the "intelligence" of intelligence tests is identical with the "achievement" of achievement tests, and vice versa; in the evidential chapter this reduces to a proof that the *variances* have this ratio. This is a Fisher technique, applied also to ratios between inheritance and environment; its conclusions are, to say the least, startling, but exactly what meaning is to be attached to it has so far escaped me. At any rate, it is food for thought in this case; we have an intelligence test, say the Binet, yielding results that seem to reflect innate differences surprisingly—as, for instance, in the Burks foster-children study; we have an achievement test, say the Stanford, which satisfies very well the most rigorous mathematical criteria known, and which has proved very effective in guiding instruction of the type we now have; yet the abilities measured by these two tests have nine times as much in common as they have of difference.

Why, exactly, do we have schools?

RAYMOND R. WILLOUGHBY

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[1] In the "Nature *vs.* Nurture."

Briffault, R. *The Mothers.* New York: Macmillan, 1927. 3 vols. Pp. xix + 781; xx + 789; xv + 841. $27.00.

This is one of those monumental works that appear from time to time as the result of almost a life-time of industry, toil, and perseverance. Some men seem to look at problems only from a monumental point of view, and express themselves in two, three, or more large volumes; while other men are able to approach the same problems in a casual manner and discuss them in brief monographs. I suppose it is a matter of temperament more than it is a difference in the way in which men see problems. I do not find, as an individual, that I learn any more from the monumental than I learn from the more concrete, more brief treatises.

These three volumes consist of approximately twenty-three hundred pages —a massive production. The work is of great intrinsic value, however, to those whose interest and leisure are great enough to permit of so great an expenditure of time.

This work first came to my attention through conversations with the late Edward Bradford Titchener. Professor Titchener had known for some years of the growth of this work and it was through his influence that the work was finally published in this country. Professor Titchener was extraordinarily interested in the value of the work which Briffault was doing. Professor Titchener, himself, was planning to review these volumes, but his death prevented a discussion of these volumes in the light of the many years of research into anthropology which Professor Titchener had expended. Titchener's friends had known for some years that his interest in psychology was waning, and that his interest in anthropology was waxing and growing warmer year by year. Just a few months before his death, Titchener told me that he considered this work of Briffault's one of the most important that was now going on in the field of anthropology. Certainly I can give no greater compliment to this massive work than merely to state that opinion of Titchener's.

The first volume begins with a discussion of heredity, of the evolution of motherhood, and of the origin of love. It proceeds to a detailed analysis of the primitive human group, and of the status of motherhood in primitive society. There follows a long chapter on the matriarchal phase in civilized societies. There is a chapter on the primitive division of labor between the sexes, and a long chapter on the institution of marriage. The volume ends with two chapters on group-marriage and sexual communism.

Volume 2 begins with chapters on promiscuity and individual marriage, primitive jealousy and love, and of primitive methods of obtaining a mate. There is a long and very important chapter on the social evolution of monogamy, and chapters on tabu, the totem, the witch, and the lord of women. The concluding chapters concern resurrection, life, and primitive cosmic religion.

Volume 3 begins with a chapter on the magical origin of queens, followed by an important chapter on "The Great Mothers." These are followed by chapters on such concepts as holy matrimony, modesty, purity, and romance. The closing chapter is about "The Mothers."

It is the author's thesis that civilization is a product of patriarchal societies and of the masculine intellect, feminine intelligence not being realistic in character, but that civilized societies are derived from non-intellectualistic, irrational, magical societies. It is his thesis that all societies are dependent upon the bonds of social sentiments and that such sentiments are derived from the maternal instincts. The author points out the impossibility of woman's full participation in the intellectualized products of patriarchal culture. He points out that the selection of masculine ideals is dependent

upon woman and that mothers are the real rulers of human destiny.

A striking contribution of Briffault's whole work lies in the distinction which he draws between civilization and civilized societies; civilization being man-made, and civilized societies being woman-made. The distinction is of great importance to sociology, anthropology, and organized religion. Surely, civilized societies will survive long after civilization may have passed away.

CARL MURCHISON

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Griffith, C. R. *General Introduction to Psychology.* (Rev. Ed.) New York: Macmillan, 1928. Pp. xix + 607. $2.75.

It has been evident for some years that there is no general agreement concerning what a textbook in psychology should consist of. A satisfactory solution of this difficulty will do more to determine the future history of psychology as a science, than will the solution of any other single problem now being considered by professional psychologists. I do not believe that any one individual will ever write such a textbook. Each individual carries with him too many limitations and too many prejudices to make such a task possible even if the necessary knowledge were possessed. No satisfactory definition of the field of psychology will ever be possible as long as the subject-matter of psychology is allowed to contain material that has only a limited recognition of being experimental. Both the structuralists and the behaviorists have given to psychology volumes of data labelled by their authors as being experimental but which are not recognized by psychologists in general as being truly experimental within the field of psychology. The difficulty is that most of us are working on problems that do not exist and which must be rooted up and thrown out of psychology before scientific advancement is possible. Psychology as a science is suffering from the tuberculosis of "perception," from the hydrophobia of "imagination," from the scurvy of "the feelings," and from the dry-rot of "instinct." All of these problems must be thrown out of psychology before it can ever hope to be a progressive science. No textbook that seriously and systematically discusses such problems can ever hope to be more than a transient bit of literature.

Dr. Griffith's book is extremely modern in that it browses over all the present-day fields of psychology. The book is divided into the following five parts: "The Foundations of Psychology," "Genetic Psychology," "Social Psychology," "Psychology of the Unusual and of the Abnormal," and "Psychotechnology." The book seems to have grown from a careful reading of all the textbooks of psychology published in the English language during the last quarter of a century. Each one of the five parts is divided into a number of chapters, and it is safe to say that no topic in modern psychology is overlooked. The book is wholly and completely different in subject-matter and method of treatment from anything that would be written by a structuralist or a behaviorist. It is also wholly different from the type of thing Hollingworth has recently done. It is the work of a well-trained, highly intelligent reporter. No topic has been overlooked, no news-item has escaped notice, everything is mentioned, everything is discussed, and discussed well.

It is a tragedy that so many psychologists spend so much energy in putting into print and fixing, for the time being, so many things that have no business in psychology. The task of debunking the whole field of psychological interest becomes more difficult as such books multiply. More than a dozen

first-class textbooks in psychology have been written in the last four or five years that never should have seen the light of day. They give a wholly distorted and erroneous impression of what psychology is all about, and what psychology can do.

If one should be limited to the use of a single textbook in conducting an elementary class, and the instructor's plan should be to give as wide a survey as possible of the field of psychology, this volume should prove most excellent. But what a day of reckoning that will be when a student who has been brought up on this type of book goes to the graduate school and finds that he must discard about nine-tenths of the data of his early training!

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Gates, G. S. *The Modern Cat: Her Mind and Manners.* New York: Macmillan, 1928. Pp. xvi + 196. $2.00.

Mrs. Gates has done for the cat what Yerkes did some time ago for the chimpanzee. The book is very interestingly written and contains a very wide range of subject-matter. There are startling contrasts which bring up the reader with amazement at times. Most charming stories concerning the cats of literary people are told with delicacy and wit. These are interspersed with lines of poetry which really entertain. Alongside the tales and the poetry, we find experimental data in scientific language. Such a combination is not bad and breaks the monotony with delightful contrasts. But alongside the tales, poetry, and science, we find flights of poetic fancy which the author may not have intended as such. She talks about the cat's universe being colorless and toneless! Whether this universe is thought of by the author as being another old tale or bit of poetry, or as a scientific concept, it is impossible to determine. It reminds one somewhat of the comical fairy-tale world that Koffka sees through the eyes of an infant and which he describes in his "Growth of the Mind."

The book is excellent in its form and in its aim. In every chapter the author insists on scientific behavioristic technique, but in many of the interpretations of the results of this technique, she is bound by no law more stringent than that of literary charm.

The book is really excellent for the non-scientific reader. It demolishes or carefully dissects many traditional beliefs concerning the behavior of cats, and attempts to make of the book an elementary introduction to comparative psychology. There is plenty of room for a book of this sort, and it can serve a most useful purpose. There are millions of non-scientific individuals who would greatly profit and, at the same time, be entertained by reading this book.

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BRIFFAULT, R. The mothers. New York: Macmillan, 1927. 3 vols. Pp. xix + 781; xx + 789; xv + 841. $27.00. (Reviewed in this number.)

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PRESENT-DAY TENDENCIES IN RUSSIAN PSYCHOLOGY*

From the Bekhterev Refleksologicheskii Institut po Izucheniya Mozga

A. L. Schniermann

The present state of psychology in U. S. S. R. can be characterized as a full growth of materialistic tendencies in the study of human personality and of behavior. Some of these tendencies arose within the last few years, others are of a rather long standing.

1. Old Objective Schools

Russia as well as the United States may be considered as the cradle of objective tendencies in psychology. Already at the beginning of this century we find two scientific schools initiating and developing an objective study of behavior. The two schools, though of different origins, coincide in some points.

The School of I. P. Pavlov

This school can be defined as entirely physiological. In the year 1863 the famous Russian physiologist, J. M. Sechenov, offered the hypothesis concerning the reflective functions of the cerebral hemispheres. At the beginning of this century I. P. Pavlov began an experimental analysis of these functions. His method, known under the title of "Conditioned Reflexes," consists in making a study of the formation of salivary-secretion reflexes in correlation with different outward irritations. An unconditioned excitor of the salivary-secretion (some acid, biscuit, meat-powder, etc.), added to an indifferent irritator (sound, light), transforms the latter into a "conditioned excitor" of the same secretion. The laws of the excitation and inhibition processes taking place in the central nervous system, their irradiation, concentration, induction, etc., are revealed by the study of the formation, growth, and generalization of the new "conditioned reflexes," on the one hand, and of the extinguishing, retarding, "differentiating inhibition," on the other hand. This study makes possible an analysis of some of the general characteristics of the nervous system, e. g., the mutual effectiveness of the excitation and inhibition processes (phenomenon of sleep, phenomena of neurosis, disturbance of the equilibrium of these processes). A planned and continuous study of the functional laws of the hemispheres (and lately the study of the subcortical centers of the brain) made it possible for Pavlov's school to accumulate a large number of facts which reveal an objective regularity in the ground of many complicated acts of

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animal behavior. When speaking of human behavior, Pavlov ventures to interpret some cases by analogy with the same laws obtained through the study of animals.

Some of his pupils attempted to extend this analogy to the most complicated acts of human behavior (for instance, creative power) and even to some phenomena of social life. However, these attempts cannot be considered as successful. Some other pupils attempted to experiment directly on persons. Only lately, with the invention of Lashley's funnel, has it proved possible to adapt Pavlov's principal method of conditioned salivary-secretion reflex. These investigators were induced therefore to use other methods, principally the method of motor reflexes, belonging to the school of V. M. Bekhterev. Nevertheless one can say that the work with human material in this school is as yet in a primitive stage.

The School of V. M. Bekhterev

Human personality is the chief and direct object of study in this school. V. M. Bekhterev, neurologist and psychiatrist, began to study the localization of cerebral functions in the last quarter of the past century. In addition to making clinical observations and patho-anatomical investigations, he possessed also the experimental method, especially the method of extirpation of various sections of animal brains. His early investigations concerned innate or naturally acquired reactions. Within the first ten years of our century Bekhterev initiated the study of specially trained "associated reflexes" under laboratory conditions, beginning the work on dogs (V. P. Protopopov) and continuing on human material. The method of "associated reflexes" differs from the method of "conditioned reflexes" in that the former uses the electrical skin-irritation as the chief irritator ("unconditioned irritator" with Pavlov). This electrical skin-irritator provokes a defensive movement in one of the extremities. When combined with some originally indifferent irritator, it gives place to an "associated ('conditioned' with Pavlov) reflex," the latter irritator ("associated" or "conditioned irritator") provoking the same defensive movement of the extremity.

The selection of the motor sphere instead of the secretory offers many advantages. It facilitates the transfer of the experiments upon human material. It does not exclude from the area of study (as when working with the method of salivary-secretion reflexes) the numerous group of so-called voluntary acts (acts under control). It makes it possible to account not only for the positive, but also for the negative values of the reflex, the effect of the antagonists.

It is not only the difference in the principal method of investigation and in nomenclature which separates Bekhterev's school from the school of I. P. Pavlov. There are some inner divergencies which are connected with the very origin of these schools. If Pavlov's

teaching, which arose from the depths of physiological laboratories and aspires to an exact, to an almost mathematical analysis, must be characterized as entirely physiological, Bekhterev's teaching, created by this psychoneurologist of wide conception and of great social capacities, must be defined as biosociological.

There is no room in this paper in which to give a detailed history of this biosociological conception of personality. The sources from which it arose have found their adaptation in those different disciplines which are forming "Reflexology" (this is the name Bekhterev gave to his teaching about human personality). These disciplines indicate the characteristics of Bekhterev's "Reflexology" which differentiate it from Pavlov's "Physiology of the Cerebral Hemispheres."

Even in the most physiological part of Reflexology (analysis of experimental investigations) we have to account as much for the work of the hemispheres as for the work of subcortical and other underlying centers, the vegetative nervous system, the endocrine glands, etc. But even the utmost detailed physiological analysis of the reflexological experimental investigations cannot exhaust Reflexology as a branch of biology; only an evolutional conception of the question gives to Reflexology the right to bear this name. Therefore, Reflexology extends its investigations to the ontogenesis and phylogenesis of all outward manifestations of human personality, forming simultaneously with General Reflexology a special discipline known as Genetic Reflexology. Yet these two disciplines together do not exhaust the reflexological conception. As a biosociological conception Reflexology must render an account not only of the biological elements, but also of the social components of behavior. Reflexology studies these components as they appear in ontogenesis (a part of Genetic Reflexology), investigates individual and age characteristics (Individual and Age Reflexology), and makes a study of human behavior when acting in common (Collective Reflexology). Individual and Age Reflexology, as well as Genetic Reflexology, consists of biological and sociological elements.

Thus the school of Bekhterev requires a very broad conception indeed of the behavior problem and contains a series of special disciplines. Only the objective study of personality in its outward manifestations unites this school with the school of I. P. Pavlov and at the same time separates these two schools from all other psychological tendencies.

II. New Objective Tendencies in Psychology

Materialistic ideology was always very much cultivated among Russian educated classes. Yet the old mechanical, naïve materialism cannot meet the requirements of a severe critic. Disillusioned in such materialism, many people proceed to the other extreme—an unrestrained idealism, even solipsism.

The social revolution brought an ideological revolution, upsetting the idealistic tendencies. The ideological war is not yet finished, but one can undoubtedly say that materialism (not the old naïve one, but the new dialectic materialism) has won a stable and prevalent position. This fact cannot remain without consequences for those Russian psychological schools which stood previously on a subjective platform built upon an idealistic conception. Most of these subjective tendencies were forced to undergo a more or less important evolution.

The School of K. N. Kornilov

It is of interest to notice that the exceedingly idealistic school of G. J. Chelpanov has undergone the greatest evolution. Issuing from the teaching of the immortality of the soul, this school came almost to a reflexological conception of behavior. But in order to realize this evolution it had to outgrow its creator and even to dismiss him. At the head of this new teaching which has arisen from the ruins of the old Chelpanov's idealistical school stands K. N. Kornilov. Under his guidance this new psychological teaching entered the zone of objective experimental investigations, and at the same time has undertaken a radical analysis of its ideological position.

Being a materialist-dialectist, Kornilov considers the subjective correlates as the introspective side of the physiological material-processes taking place in the nervous system (the principle of unity of the physical and psychical, yet not their identity). Hence he thinks it possible to establish a causal coherency between physical phenomena, but never between physical and psychical nor between two psychical phenomena. As far as the purpose of the science includes not only a *description,* but also an *explanation* of phenomena, Kornilov comes to the conclusion that only the objective method which reveals the causal coherency between phenomena can be considered as entirely scientific, while the method of introspection offers only a subjective description, which, without a control of objective data, is of no scientific value.

But in spite of this statement, which relates Kornilov to the reflexological platform, he could not give up the name "psychology" and renounce the study of subjective phenomena, because such an abandonment would signify a fall into agnosticism. Therefore Kornilov's school studies simultaneously with the objective elements the subjective correlates in the form of the "verbal reports" of the subjects. Yet these subjective elements diminish in importance and Kornilov's school comes closely to Reflexology, the two having in common the biosocial analysis.[1]

[1]But in this analysis Bassov's school gives a prevalent place to social elements.

Other Objective Psychological Tendencies

A lot of other psychological schools show the same aspiration to embrace in a common dialectic a materialistic conception both of subjective and objective elements. Sometimes one can see the influence of certain West-European schools (*Gestaltpsychologie*), but in general, Russia, being for a long time isolated from all other countries, has had to follow her own way in psychology as well as in other sciences.

Most of the "objective-psychological" tendencies are of an empirical character. As an example we can name the school of M. J. Bassov, a pupil of the late A. F. Lazurski.[2] Bassov's school studies the objective manifestations of children's behavior as well as the subjective correlates, trying to realize a harmonious conception of this study. Yet the chief result in the work of this school appears in an empirical description, because Bassov prefers to forego the analysis of the data received by means of observations and of physiological experiments. The reason for such a refusal is quite comprehensible: the physiological analysis excludes the psychological analysis, which it is difficult to give up. Yet the psychological analysis deprives one of the materialistic basis of the causal conception. Therefore Bassov's school, like most of the psychological schools bearing an "objective tendency," accumulates rich empirical material which always contains a certain percentage of subjective elements, and puts aside the question concerning the analysis of this material.

III. Comparative Psychology or Biopsychology

Amid the different psychological teachings which are trying to embrace the subjective and objective elements of behavior, it is quite necessary to mention a rather old school of "comparative psychology" or "biopsychology." This school is working under the ideological guidance of the eminent biologist W. A. Wagner.

Annotating a rather great dispersion of different psychological schools, Wagner undertakes to unite them under the term "general psychology," just as biology has been united under the term "general biology." He offers to combine the chief results of all existing schools, maintaining their methods, and to classify these results from the point of view of the evolution theory. The purpose of "general psychology" consists of the study of the evolution of psychical capacities. A special method of investigation is necessary for each stage of evolution. Thus methods of Loeb's physical-chemical school are adapted to the first stage of prenervous animal life; physiological methods to the second stage of nervous prepsychical life; biopsycho-

[2] A. F. Lazurski was an eminent pioneer of the psychological experiment characterology in Russia. He already belonged to Bekhterev's school before the creation of Reflexology.

logical, physiological, and morphological methods, as well as the method of behaviorism, are applied to the third and fourth stages—the development of instinctive and emotional capacities; in the fifth stage, human biopsychology, all the above-mentioned methods, especially the method of introspection and of experimental psychology; and in the sixth stage, collective psychology, all the above methods as well as history, economy, ethnology, etc.

It is quite evident that this school, including some elements of subjectivism which is transferred even to animal behavior (emotions, instincts), brings to the analysis of behavior, on a par with causal conception, some teleological elements. Thus this school as well as other psychological teachings which include in their investigations some subjective elements, bears a certain print of eclecticism.

It does not answer the purpose of this paper to mention all existing Russian psychological schools and tendencies. Many of them do not differ much from one another. Our purpose was to give a short report of certain outstanding characteristics of present-day Russian psychology. Yet one can assert that in nearly all Russian psychological schools one can observe the tendency towards an objective materialistic conception of behavior. The struggle is not yet finished. The idealistic tendencies burst forth here and there, but life itself classifies psychology as a strictly objective science.

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TENDANCES D'AUJOURD'HUI DANS LA PSYCHOLOGIE RUSSE

(Résumé)

On peut dire que la psychologie russe d'aujourd'hui est matérialiste. Cette tendance matérialiste dérive de deux sources principales. La première source, c'est les anciennes écoles objectives de psychologie représentées par Pavlov et Bekhterev. Pavlov, employant la méthode des réflexes conditionnés, a essayé d'établir une corrélation des réflexes de sécrétion salivaire avec divers stimuli extérieurs et a établi une régularité objective dans les actes compliqués du comportement animal. Bekhterev, employant la méthode des réflexes associés, a essayé une corrélation des mouvements défensifs des extrémités organiques initiés par une stimulation électrique de la peau avec des stimuli originairement indifférents quand on ne donne pas la stimulation électrique.

La seconde source principale, c'est l'ancienne école idéaliste de Chelpanov basée d'abord sur la conception de l'immortalité de l'âme. Cet ancien idéalisme sous la direction de Kornilov a essayé d'établir le principe qu'il y a une cohérence causale entre les phénomènes physiques, mais jamais entre les phénomènes physiques et psychiques, ou entre ceux seulement psychiques. L'école de Kornilov étudie les corrélations subjectives sous la forme des témoignages verbaux des sujets, mais ne considère pas ces corrélations d'une grande importance scientifique excepté comme exemples du côté introspectif des processus physiologiques du système nerveux.

L'école de Bassov, occupée de l'étude du comportement des enfants, amasse beaucoup de données empiriques, mais ces données ne sont jamais analysées.

La psychologie comparative sous l'influence de W. A. Wagner essaie d'unir toute la psychologie sous le terme "psychologie générale" et d'étudier les problèmes psychologiques au point de vue de l'évolution ou au point de vue génétique.

Il considère que le développement psychique s'avance à travers diverses phases, s'étendant depuis la première phase de la vie animale pré-nerveuse jusqu'à la sixième phase du comportement humain collectif.

La psychologie russe dérive principalement des origines matérialistes et est devenue plus matérialiste parceque les écoles idéalistes aussi sont devenues matérialistes.

SCHNIERMANN

TENDENZEN DER RUSSISCHEN PSYCHOLOGIE DER GEGENWART

(Referat)

Man kann wohl die heutige Psychologie in Russland als materialistich bezeichnen. Diese materialistische Tendenz ist zwei Quellen entsprungen. Der erste Ursprung besteht in den alten, objectiven Schulen der Psychologie, deren Repräsentanten Pavlov und Bekhterev waren. Pavlov, unter Anwendung der Methode der bedingten Reflexe, versuchte eine Korrelation der Speichelabsonderungsreflexe mit verschiednen äusseren Reizmitteln darzulegen, und erwies eine objective Regelmässigkeit in den komplizierten Handlungen des Tierverhaltens. Bekhterev, mit der Methode assoziierter Reflexe, versuchte eine Korrelation der Abwehrbewegungen organischer Extremitäten, bei elektrischen Hautreizen, mit ursprünglich wirkungslosen Reizmitteln, wenn der ursprüngliche, elektrische Reiz zurückgehalten wird, festzustellen.

Der zweite Hauptursprung ist die alte, idealistische Schule Chelpanov's, welche ursprünglich auf den Glauben an die Unsterblichkeit der Seele begründet war. Dieser alte Idealismus, mit Kornilov an der Spitze, versuchte das Prinzip festzustellen, dass eine kausale Beziehung zwishen psychischen Erscheinungen besteht, doch nie zwischen physischen und psychischen, oder zwischen physischen an und für sich. Kornilov's Schule untersucht subjective Korrelate in der Form von Aussagen der Versuchspersonen, ohne diesen, jedoch, grosse wissenschaftliche Bedeutung beizulegen, ausser als Beispiele des introspectiven Teils der physiologischen Vorgänge, welche in dem Nervensystem stattfinden.

Bassov's Schule, die das Verhalten der Kinder untersucht, sammelt eine Menge empirisches Material, doch wird dasselbe nie analysiert.

Die vergleichende Psychologie, unter W. A. Wagners Einfluss, macht den Versuch, die ganze Psychologie, unter dem Titel "Allgemeine Psychologie," zu vereinigen, und die psychologischen Probleme, vom genetischen oder Evolutionsstandpunkt aus, zu studieren.

Seiner Ansicht nach, hat die psychische Entwicklung verschiedne Stadien durchgemacht, von der ersten Stufe des nervenlosen Tierlebens, bis zu der sechsten Stufe des Gesammtverhaltens des menschlichen Wesens. Die russische Psychologie hat sich hauptsächlich aus materialistischen Quellen entwickelt, und ist um so realistischer geworden, da selbst idealistische Schulen sich schliesslich dem Materialismus zugewandt haben.

SCHNIERMANN

СОВРЕМЕННЫЕ ТЕНДЕНЦИИ В РУССКОЙ ПСИХОЛОГИИ

(Реферат).

Современная Психология в России может быть охарактеризована как материалистическая. Эта материалистическая тенденция вытекает из двух главных источников: первым являются старые об'ективные школы Психологии, возглавляемые Павловым и Бехтеревым. Павлов пользуясь методом условных рефлексов и ища корреляции между слюно-отделительными рефлексами и различными внешними стимулами, установил об'ективную закономерность в сложных актах поведения животных. Бехтерев, пользуясь методом сочетательных рефлексов, приступил к выработке корреляции между защитными движениями органических конечностей, вызываемыми электрическим раздражением кожи и первоначально индифферентными стимулами, когда первоначальное электрическое раздражение не применяется. Вторым главным источником является идеалистическая школа Челпанова, базирующаяся первоначально на понятии бессмертия души. Этот старый идеализм под руководительством Корнилова пытается установить принцип, что существует причинная связь между физическими явлениями, но не между физическими и психическими, или между одними психическими. Школа Корнилова изучает суб'ективные корреляты в форме словесных отчетов испытуемых, но не считает эти корреляты большой научной важности, разве только как примеры интроспективной стороны физиологических процессов, происходящих в нервной системе. Школа Бассова, занимаясь изучением поведения детей, накопляет большое количество эмпирического материала, никогда не анализируемого.

Сравнительная Психология, под влиянием Вагнера, пытается об'единить всю Психологию под термином «Всеобщая Психология» и изучает психологические проблемы с эволюционной или генетической точки зрения. Он рассматривает физическое развитие как проходящее через различные стадии, начиная с первой стадии до-нервной животной жизни до шестой стадии коллективного поведения человека.

Русская Психология, имея главным образом материалистическое происхождение, стала еще более материалистической благодаря тому факту, что идеалистические школы превратились также в материалистические.

ШНИРМАН.
(Schniermann)

AN EXPERIMENTAL STUDY OF NATIONALITY PREFERENCES*

From the Psychological Laboratories of the University of Chicago and of the Illinois Institute for Juvenile Research

L. L. THURSTONE†

The experiment here reported was designed as a rather severe test of the law of comparative judgment. This psychophysical law states the relation between the psychological separation of the stimuli (the sense distance) and the probability of correct discrimination on any single occasion. The law applies ideally to the situation in which a single observer makes several hundred discriminatory judgments for each possible pair of stimuli in any given stimulus series. The stimuli to which the law refers in its conventional psychophysical setting are the lifted weights, the grey papers, the line lengths, and the rest of the traditional stock-in-trade of the psychophysicist. The ideal situation above described constitutes Case I in the formulation of the law.[1] By a number of assumptions previously described, it is possible to apply the law to those situations in which a *group* of observers are used instead of a single observer and in which each member of the group makes only one discriminatory judgment for each possible pair of stimuli in the stimulus series. We shall here use the simplest form of the law, previously described as Case V.

We shall also transfer the whole psychophysical reasoning from its traditional stimuli in which the subject responds to the question, "Which of these cylinders is the heavier?" (apologies for the stimulus error!) to the question, "Which of these two nationalities would you generally prefer to associate with?" To transfer the application of the law from lifted weights to nationality preferences introduces

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I wish to make special acknowledgment to my research assistant for the Local Community Research Committee, Miss Annette McBroom, who has carried out all of the calculations in this study. Her part in the study has been much more than that of a statistical assistant, for she has made many valuable suggestions regarding the handling of the data. For help in the preparation of the mimeographed forms and in the collection of the original data I wish to acknowledge the effective work of Mr. W. H. Cowley, who was at the time research assistant at the Institute for Juvenile Research.

[1]Thurstone, L. L. A law of comparative judgment. *Psychol. Rev.*, 1927, **34**, 273-286.

new factors into the judgments. The nationality preferences are saturated with prejudice and bias, with religious affiliations, and with wide differences in knowledge and familiarity. In addition to these variable factors, the group of 239 subjects was also intentionally left as heterogeneous as it was found to be among undergraduates at the University of Chicago. It is clear that with large proportions of Jews, Catholics, Negroes, Protestants, and students whose parents are foreign-born, it may be very questionable whether the distribution of attitude toward any one of the twenty nationalities is in any sense normal on the psychological continuum. However, when the calculated proportion of each judgment is compared with the corresponding experimental proportion, the average discrepancy is found to be only .03, which shows rather convincingly that the law of comparative judgment can be used for measuring not only the formal "sense distance" of conventional psychophysics but even so complex a continuum as the degrees of preference of a heterogeneous group of subjects for nationalities and races.

The experiment was performed by asking each of the 239 undergraduates to fill in a mimeographed schedule showing their racial and nationality preferences. The instructions on the schedule were as follows:

AN EXPERIMENTAL STUDY OF RACIAL ATTITUDES

This is an experimental study of attitudes toward races and nationalities. You are asked merely to underline the one nationality, or race, of each pair that you would rather associate with. For example, the first pair is:

Englishman—South American

If, in general, you prefer to associate with Englishmen rather than with South Americans, underline Englishman. If you prefer, in general, to associate with South Americans, underline South American. If you find it difficult to decide for any pair, simply underline one of them anyway. If two nationalities are about equally well liked, they will have about the same number of underlinings in all of the papers. Be sure to underline one of each pair even if you have to make a sort of guess.

American—Hindu
Englishman—Swede
Japanese—Italian
Hindu—Frenchman
Mexican—American
etc.

The list of pairs was continued so that every one of the twenty-one races or nationalities had been compared with every other one. This required

$$\frac{n(n-1)}{2} = 210$$

judgments which were mimeographed on three letter-sized sheets. The complete list of nationalities and races is given in the tables. In addition to these 210 judgments there were 21 repetitions, making a total of 231 pairs on the mimeographed forms. The purpose of the 21 repetitions was to verify empirically the probable errors of the proportions so that some estimate might be made of the probable error of the resulting scale values, both theoretically and empirically.

The law in the complete form is as follows:

$$S_1 - S_2 = x_{12}\sqrt{\sigma_1^2 + \sigma_2^2 - 2 \cdot r_{12} \cdot \sigma_1 \cdot \sigma_2} \qquad (1)$$

in which

$(S_1 - S_2)$ = sense distance, i. e., the scale distance between the two modal discriminal processes.[2]

x_{12} = the sigma value corresponding to the observed proportion of judgments "R_1 is greater than R_2." R denotes the stimulus. The proportion is designated $P_{1>2}$. The numerical value of x_{12} is positive when $P_{1>2}$ is greater than .50, and it is negative when $P_{1>2}$ is less than .50.

σ_1 and σ_2 = ambiguities (discriminal errors) of the two stimuli, R_1 and R_2.

r_{12} = the correlation between the discriminal deviations involved in the same judgment.

In the present study we have used Case V in which the law is applied to a group of observers, each observer giving only one judgment for every stimulus pair. In Case V it is also assumed that $r_{12} = 0$ and that the value of σ is unity for every stimulus in the series. With these approximations the law takes the simpler form

$$S_1 - S_2 = x_{12}\sqrt{2} \qquad (2)$$

when σ is chosen as the unit of measurement.

The first results are shown in Table 1. The interpretation of this table may be seen perhaps best by the following direct readings. Of the whole group, 89.8% preferred to associate with Americans rather than with Englishmen; 38.0% preferred to associate with Chinamen rather than with Japanese; 37.2% preferred to associate with Russians rather than with Italians. The rest of the table is interpreted in the same manner. These constitute the data on which our measurements will be based.

Our object is to calculate a scale value for each of the 21 nationalities. With these 21 scale values we shall prepare a table of 210 calculated proportions. The discrepancies between these two tables will be the criterion by which to judge the degree to which the law of comparative judgment is applicable to this type of data.

[2]For the psychophysical terminology here used see: Thurstone, L. L. A mental unit of measurement. *Psychol. Rev.*, 1927, **34**, 415-423.

TABLE 1

EXPERIMENTAL PROPORTIONS

	American	Armenian	Chinaman	Englishman	Frenchman	German	Greek	Hindu	Irishman	Italian	Japanese
American	—	.000	.000	.102	.046	.042	.004	.004	.030	.013	.000
Armenian	1.000	—	.342	1.000	.983	.912	.545	.270	.975	.782	.481
Chinaman	1.000	.658	—	.992	.954	.950	.675	.447	.987	.882	.620
Englishman	.898	.000	.008	—	.114	.133	.008	.013	.244	.030	.000
Frenchman	.954	.017	.046	.886	—	.398	.013	.042	.622	.222	.021
German	.958	.088	.050	.867	.602	—	.029	.034	.644	.142	.050
Greek	.996	.455	.325	.992	.987	.971	—	.275	.970	.837	.302
Hindu	.996	.730	.553	.987	.958	.966	.725	—	.971	.878	.664
Irishman	.970	.025	.013	.756	.378	.356	.030	.029	—	.063	.054
Italian	.987	.218	.118	.970	.778	.858	.163	.122	.937	—	.285
Japanese	1.000	.519	.380	1.000	.979	.950	.698	.336	.946	.715	—
Jew	.953	.380	.206	.862	.792	.761	.397	.186	.779	.631	.263
Mexican	.992	.621	.336	.992	.987	.975	.660	.399	.983	.916	.536
Negro	.996	.796	.691	.996	.975	.975	.853	.687	.983	.933	.744
Pole	1.000	.428	.291	.975	.983	.886	.452	.208	.954	.808	.360
Russian	.983	.359	.189	.962	.932	.886	.350	.170	.904	.628	.245
Scotchman	.966	.046	.017	.722	.357	.348	.030	.008	.532	.076	.013
Spaniard	.992	.208	.122	.987	.954	.858	.259	.154	.917	.517	.201
South American	.992	.192	.114	.962	.801	.828	.213	.130	.870	.473	.114
Swede	.983	.109	.055	.878	.611	.674	.084	.102	.744	.200	.084
Turk	1.000	.820	.641	.996	.987	.983	.787	.664	.983	.954	.735
Σp	19.616	6.669	4.497	17.884	15.158	14.710	6.975	4.280	15.975	10.700	5.772
Rank order	1	15	18	2	5	6	14	19	4	9	16

TABLE 1 (*continued*)

	Jew	Mexican	Negro	Pole	Russian	Scotchman	Spaniard	South American	Swede	Turk	Check Column
American	.047	.008	.004	.000	.017	.034	.008	.008	.017	.000	.384
Armenian	.620	.379	.204	.572	.641	.954	.792	.808	.891	.180	13.331
Chinaman	.794	.664	.309	.709	.811	.983	.878	.886	.945	.359	15.503
Englishman	.138	.008	.004	.025	.038	.278	.013	.038	.122	.004	2.116
Frenchman	.208	.013	.025	.017	.068	.643	.046	.199	.389	.013	4.842
German	.239	.025	.025	.114	.114	.652	.142	.172	.326	.017	5.290
Greek	.603	.340	.147	.548	.650	.970	.741	.787	.916	.213	13.025
Hindu	.814	.601	.313	.792	.830	.992	.846	.870	.898	.336	15.720
Irishman	.221	.017	.017	.046	.096	.468	.083	.130	.256	.017	4.025
Italian	.369	.084	.067	.192	.372	.924	.483	.527	.800	.046	9.300
Japanese	.737	.464	.256	.640	.755	.987	.799	.886	.916	.265	14.228
Jew	—	.282	.090	.489	.487	.809	.610	.586	.753	.149	10.465
Mexican	.718	—	.282	.720	.739	.992	.903	.823	.941	.254	14.769
Negro	.910	.718	—	.832	.881	.987	.937	.928	.962	.597	17.381
Pole	.511	.280	.168	—	.583	.954	.655	.688	.881	.151	12.216
Russian	.513	.261	.119	.417	—	.921	.669	.580	.799	.090	10.977
Scotchman	.191	.008	.013	.046	.079	—	.046	.092	.201	.000	3.791
Spaniard	.390	.097	.063	.345	.331	.954	—	.517	.754	.029	9.649
South American	.414	.177	.072	.312	.420	.908	.483	—	.765	.042	9.282
Swede	.247	.059	.038	.119	.201	.799	.246	.235	—	.026	6.494
Turk	.851	.746	.403	.849	.910	1.000	.971	.958	.974	—	17.212
Σp	9.535	5.231	2.619	7.784	9.023	16.209	10.351	10.718	13.506	2.788	
Rank order	11	17	21	13	12	3	10	8	7	20	

The first step is to ascertain the rank orders of the 21 nationalities. They are obtained by a simple summation of the proportions. These summations are given at the foot of each column in Table 1. The last row gives the absolute rank orders. The rank orders are: (1) American; (2) Englishman; (3) Scotchman, and so on.

These rank orders constitute a description of the group of undergraduates and it would vary from one group to another. In order to use this procedure for any interpretation regarding racial and nationality preferences, it is of course necessary to have a rather homogeneous group. Such a study is now being made with a large group of American-born students with American-born parents, and for several other rather homogeneous groups, similarly defined. Our present object is to test the measurement methods and it is a better test of our procedure to see how much internal consistency can be obtained with a heterogeneous group.

Some brief description of the group of undergraduates is in order because of the fact that the scale values of the nationalities naturally reflect the make-up of the group of subjects. Table 2 shows the representation of the principal countries among the 249 subjects.

TABLE 2

DESCRIPTION OF EXPERIMENTAL GROUP

Country of birth	Subject	Father	Father's father	Father's mother	Mother	Mother's father	Mother's mother
United States	.941	.628	.406	.397	.682	.423	.439
Canada	.017	.046	.013	.021	.038	.025	.021
England	.008	.042	.092	.096	.008	.079	.088
Germany	—	.059	.159	.138	.063	.151	.142
Ireland	—	.017	.063	.071	.021	.067	.059
Russia	.021	.100	.100	.092	.075	.088	.088

RELIGION	
Jewish	.151
Protestant	.636
Catholic	.100
None	.113

The next step is to prepare a table of corresponding sigma values. This has been done in Table 3. Since the procedure of weighting is awkward in these calculations, we have merely dropped the most

unreliable proportions, namely those above .97 and those below .03. The rest are retained and are given equal weight in the calculations. It will be seen from the results that no serious damage is done by this additional approximation. The sign of each sigma value in Table 3 is positive when its corresponding proportion in Table 1 is above .50, and it is negative when the proportion is less than .50.

Let the scale values of the two nationalities whose scale separation is to be ascertained be designated S_1 and S_2 respectively. Let any other nationality be designated S_k. Then, by (2)

$$S_1 - S_k = x_{1k}\sqrt{2} \qquad (3)$$

and

$$S_2 - S_k = x_{2k}\sqrt{2}\,.$$

Subtracting,

$$(S_1 - S_2) = \sqrt{2}\,(x_{1k} - x_{2k}).$$

Summing for all the nationalities, we have

$$n\,(S_1 - S_2) = \sqrt{2}\,\Sigma\left[x_{1k} - x_{2k}\right]$$

$$\text{or} \quad (S_1 - S_2) = \frac{\sqrt{2}}{n}\left[\Sigma\, x_{1k} - \Sigma x_{2k}\right]. \qquad (4)$$

This enables us to ascertain the scale separation $(S_1 - S_2)$ by making use of all the data. As an example of the application of the summation equation (4) Table 4 has been prepared. The first column is merely a list of the nationalities copied directly from Table 3. The second and third columns are copied directly from the columns "German" and "Swede" in Table 3.

The blank spaces in the second and third columns of Table 4 are transferred from Table 3. The entries in these spaces have been omitted from Table 3 because the proportions on which they are based are too unreliable, being either higher than .97 or lower than .03. In the fourth column of Table 4 are recorded the differences between the paired sigma values. Whenever one of the two sigma values is omitted, the corresponding difference is of course also omitted. The calculation of the scale separation between "German" and "Swede" is based therefore on the remaining sixteen pairs of sigma values as shown in the fourth column of the table. This fourth column is really inserted primarily for the purpose of check

TABLE 3

SIGMA VALUES CORRESPONDING TO TABLE 1

	American	Armenian	Chinaman	Englishman	Frenchman	German	Greek	Hindu	Irishman	Italian	Japanese
American	0.00	—	—	—1.27	—1.68	—1.73	—	—	—1.88	—	—
Armenian	—	0.00	— .41	—	—	+1.35	+ .11	— .61	—	+ .78	— .05
Chinaman	—	+ .41	0.00	—	+1.68	+1.64	+ .45	— .13	—	+1.19	+ .31
Englishman	+1.27	—	—	0.00	—1.21	—1.11	—	—	— .69	—1.88	—
Frenchman	+1.68	—	—1.68	+1.21	0.00	— .26	—	—1.73	+ .31	— .77	—
German	+1.73	—1.35	—1.64	+1.11	+ .26	0.00	—	—1.83	+ .37	—1.07	—1.64
Greek	—	— .11	— .45	—	—	—	0.00	— .60	+1.88	+ .98	— .52
Hindu	—	+ .61	+ .13	—	+1.73	+1.83	+ .60	0.00	—	+1.17	+ .42
Irishman	+1.88	—	—	+ .69	— .31	— .37	—1.88	—	0.00	—1.53	—1.61
Italian	—	— .78	—1.19	+1.88	+ .77	+1.07	— .98	—1.17	+1.53	0.00	— .57
Japanese	—	+ .05	— .31	—	—	+1.64	+ .52	— .42	+1.61	+ .57	0.00
Jew	+1.67	— .31	— .82	+1.09	+ .81	+ .71	— .26	— .89	+ .77	+ .33	— .63
Mexican	—	+ .31	— .42	—	—	—	+ .41	— .26	—	+1.38	+ .09
Negro	—	+ .83	+ .50	—	—	—	+1.05	+ .49	—	+1.50	+ .66
Pole	—	— .18	— .55	—	—	+1.21	— .12	— .81	+1.68	+ .87	— .36
Russian	—	— .36	— .88	+1.77	+1.49	+1.21	— .39	— .95	+1.30	+ .33	— .69
Scotchman	+1.83	—1.68	—	+ .59	— .37	— .39	—1.88	—	+ .08	—1.43	—
Spaniard	—	— .81	—1.17	—	+1.68	+1.07	— .65	—1.02	+1.39	+ .04	— .84
South American	—	— .87	—1.21	+1.77	+ .85	+ .95	— .80	—1.13	+1.13	— .07	—1.21
Swede	—	—1.23	—1.60	+1.17	+ .28	+ .45	—1.38	—1.27	+ .66	— .84	—1.38
Turk	—	+ .92	+ .36	—	—	—	+ .80	+ .42	—	+1.68	+ .63
	10.06	—4.55	—11.34	10.01	5.98	9.27	—4.40	—11.91	10.14	3.23	—7.39

TABLE 3 (*continued*)

	Jew	Mexican	Negro	Pole	Russian	Scotchman	Spaniard	South American	Swede	Turk	Check Column
American	—1.67	—	—	—	—	—1.83	—	—	—	—	—10.06
Armenian	+ .31	— .31	— .83	+ .18	+ .36	+1.68	+ .81	+ .87	+1.23	— .92	4.55
Chinaman	+ .82	+ .42	— .50	+ .55	+ .88	—	+1.17	+1.21	+1.60	— .36	11.34
Englishman	—1.09	—	—	—	—1.77	— .59	—	—1.77	—1.17	—	—10.01
Frenchman	— .81	—	—	—	—1.49	+ .37	—1.68	— .85	— .28	—	— 5.98
German	— .71	—	—	—1.21	—1.21	+ .39	—1.07	— .95	— .45	—	— 9.27
Greek	+ .26	— .41	—1.05	+ .12	+ .39	+1.88	+ .65	+ .80	+1.38	— .80	+ 4.40
Hindu	+ 89	+ .26	— .49	+ .81	+ .95	—	+1.02	+1.13	+1.27	— .42	+11.91
Irishman	— .77	—	—	—1.68	—1.30	— .08	—1.39	—1.13	— .66	—	—10.14
Italian	— .33	—1.38	—1.50	— .87	— .33	+1.43	— .04	+ .07	+ .84	—1.68	— 3.23
Japanese	+ .63	— .09	— .66	+ .36	+ .69	—	+ .84	+1.21	+1.38	— .63	7.39
Jew	0.00	— .58	—1.34	— .03	— .03	+ .87	+ .28	+ .22	+ .68	—1.04	1.50
Mexican	+ .58	0.00	— .58	+ .58	+ .64	—	+1.30	+ .93	+1.56	— .66	5.86
Negro	+1.34	+ .58	0.00	+ .96	+1.18	—	+1.53	+1.46	+1.77	+ .25	14.10
Pole	+ .03	— .58	— .96	0.00	+ .21	+1.68	+ .40	+ .49	+1.18	—1.03	3.16
Russian	+ .03	— .64	—1.18	— .21	0.00	+1.41	+ .44	+ .20	+ .84	—1.34	2.38
Scotchman	— .87	—	—	—1.68	—1.41	0.00	—1.68	—1.33	— .84	—	—11.06
Spaniard	— .28	—1.30	—1.53	— .40	— .44	+1.68	0.00	+ .04	+ .69	—	— 1.85
South American	— .22	— .93	—1.46	— .49	— .20	+1.33	— .04	0.00	+ .72	—1.73	— 3.61
Swede	— .68	—1.56	—1.77	—1.18	— .84	+ .84	— .69	— .72	0.00	—	—11.74
Turk	+1.04	+ .66	— .25	+1.03	+1.34	—	—	+1.73	—	0.00	10.36
	—1.50	—5.86	—14.10	—3.16	—2.38	11.06	1.85	3.61	11.74	—10.36	0.000

on the arithmetical work because it is not needed logically for the calculation.

At the foot of Table 4 the substitutions in the summation equation (4) are shown. The arithmetical check on the calculations is also self-explanatory in this case. The scale separation between these two nationalities is 0.3509.

The order in which the scale separations are calculated is given by the absolute rank orders of the twenty-one nationalities shown

TABLE 4

CALCULATION OF SCALE VALUES

German (1)
Swede (2)

	x_{1k}	x_{2k}	$(x_{1k} - x_{2k})$
American	(—1.73)	——	——
Armenian	1.35	1.23	0.12
Chinaman	1.64	1.60	0.04
Englishman	—1.11	—1.17	0.06
Frenchman	—0.26	—0.28	0.02
German	0.00	—0.45	0.45
Greek	——	(1.38)	——
Hindu	1.83	1.27	0.56
Irishman	—0.37	—0.66	0.29
Italian	1.07	0.84	0.23
Japanese	1.64	1.38	0.26
Jew	0.71	0.68	0.03
Mexican	——	(+1.56)	——
Negro	——	(+1.77)	——
Pole	1.21	1.18	0.03
Russian	1.21	0.84	0.37
Scotchman	—0.39	—0.84	0.45
Spaniard	1.07	0.69	0.38
South American	0.95	0.72	0.23
Swede	0.45	0.00	0.45
Turk	——	——	——
	11.00	7.03	3.97

$$S_1 - S_2 = \frac{\sqrt{2} \cdot (3.97)}{16} = 0.3509$$

Check: $\Sigma(x_{1k} - x_{2k}) = \Sigma x_{1k} - \Sigma x_{2k} = 3.97$

at the foot of Table 1. The first scale separation to be calculated was "American—Englishman." The second was "Englishman—Scotchman." The successive pairs are chosen from the absolute rank order of Table 1. Consequently there were twenty tables like Table 4 in the present study.

Theoretically, it should be possible to ascertain the scale separation between any two nationalities in the whole list even though they are quite far apart on the scale, but such a procedure would be based

on a small number of paired sigma values, many of them dropping out because of low reliability. If two nationalities are widely separated on the scale, there will be so many proportions of zero and 100% that the scaling cannot be satisfactorily made. A least square solution could be laid out which would take into consideration all of the proportions in the whole of Table 1 with a system of weights but the solution would be prohibitive. It would require the solution of a set of twenty normal equations with as many unknowns. Such a procedure is not feasible. For that reason the scale separations are determined for pairs of nationalities that are adjacent in absolute rank order.

TABLE 5

Scale Separations

American—Englishman	1.3413
Englishman—Scotchman	.7549
Scotchman—Irishman	.0859
Irishman—Frenchman	.2830
Frenchman—German	.0919
German—Swede	.3509
Swede—South American	.7336
South American—Italian	.0269
Italian—Spaniard	.1242
Spaniard—Jew	.1281
Jew—Russian	.1803
Russian—Pole	.3175
Pole—Greek	.2047
Greek-Armenian	.0592
Armenian—Japanese	.2572
Japanese—Mexican	.1622
Mexican—Chinaman	.2037
Chinaman—Hindu	.0474
Hindu—Turk	.4678
Turk—Negro	.0479

In Table 5 we have a list of the scale separations of the twenty pairs of nationalities which have been calculated by the procedure shown in Table 4. The next step is to adopt the scale value of one of these nationalities as an origin. The scale value of "American" was chosen as an origin and, since all other nationalities are preferred less often by our subjects, all of the other scale values are negative. In Table 6 the final scale values have been recorded. They are obtained directly from the scale separations of Table 5 with the scale value of "American" set at zero. It is of course clear that we are here dealing with a scale in which the origin is quite arbitrary. The law of comparative judgment gives the scale separation but says nothing about measurement from any origin.

In Figure 1 the scale values of Table 6 have been represented

TABLE 6
SCALE VALUES

American	0.0000
Armenian	—4.6824
Chinaman	—5.3055
Englishman	—1.3413
Frenchman	—2.4651
German	—2.5570
Greek	—4.6232
Hindu	—5.3529
Irishman	—2.1821
Italian	—3.6684
Japanese	—4.9396
Jew	—3.9207
Mexican	—5.1018
Negro	—5.8686
Pole	—4.4185
Russian	—4.1010
Scotchman	—2.0962
Spaniard	—3.7926
South American	—3.6415
Swede	—2.9079
Turk	—5.8207

graphically, but it should be borne in mind that this figure represents merely the distribution of attitude toward these various nationalities in the particular group of this study. If we should want to compare the attitudes of two groups, it would be possible to scale the same list of nationalities for both groups separately and then to correlate the two sets of scale values. A high correlation between the two sets of scale values would show objectively that the two groups have very similar attitudes toward the nationalities in question while a low correlation would indicate that the two groups are very different in their national preferences. It would, in fact, be rather interesting to make such comparisons for representative groups of students at the universities in different countries and such a study is now being planned.

If two groups were to be compared in the manner suggested above, it is conceivable that the two scales would differ considerably in the range of scale values. The present group of subjects has a range of nearly six sigma for the whole list of twenty-one nationalities. Now, if another group should have a range of, say, only three sigma, it would indicate that the latter group is more international in its attitudes, more tolerant of national differences. A wide range of scale values indicates rather strong and rather uniform national preferences. An internationally minded group would give proportions hovering more closely about .50 and hence the scale separations would be, on the whole, smaller than the scale separations for a prejudiced

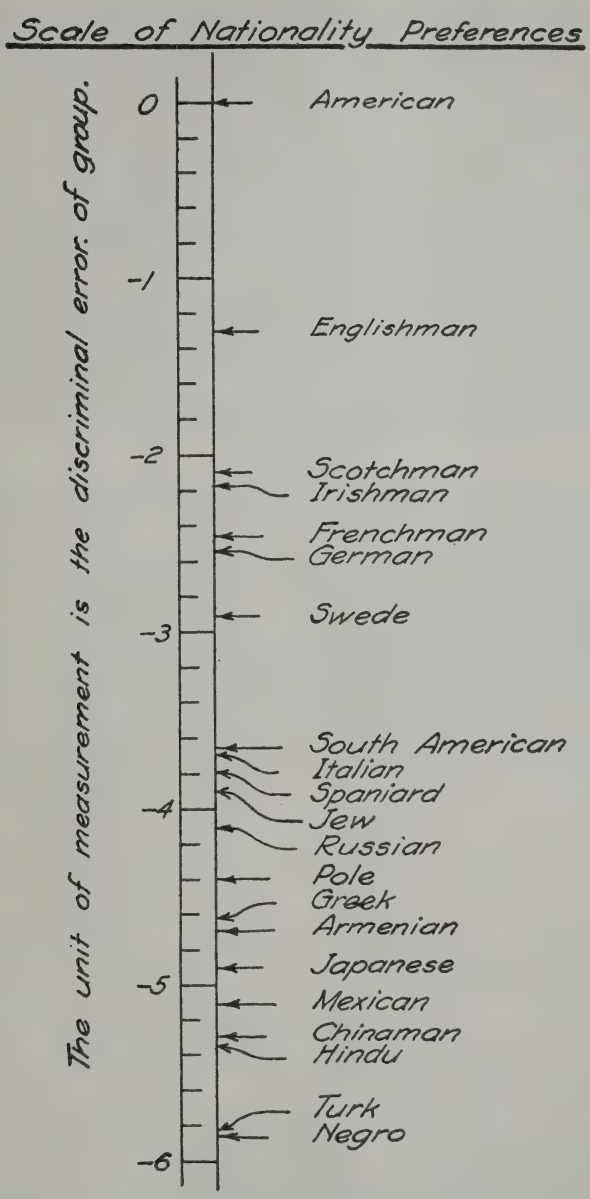
Scale of Nationality Preferences
The unit of measurement is the discriminal error of group.
0
-1
-2
-3
-4
-5
-6
American
Englishman
Scotchman
Irishman
Frenchman
German
Swede
South American
Italian
Spaniard
Jew
Russian
Pole
Greek
Armenian
Japanese
Mexican
Chinaman
Hindu
Turk
Negro

FIGURE 1

group. We have here a psychophysical tool for making objective the national attitudes of a group. It is conceivable that the method may be of some use in the objective measurement of social attitudes that are usually reported with the prejudice or bias of individual authors and investigators.

With the twenty-one scale values before us in Table 6, we want to ascertain how much internal consistency we may have in such a scale. In order to test for internal consistency we shall use the scale values as the basis for a set of calculated proportions which may be compared with the experimental proportions from which we started. We return to equation (2). With the scale values known we may calculate the value of x_{12} and this may be converted into the corresponding proportion by the aid of a probability table. This will be the calculated proportion which is to be compared with the corresponding experimental proportion. An example of the calculations is shown in Table 7. The first column contains the list of nationali-

TABLE 7

CALCULATION OF DISCREPANCIES FOR COLUMN "SWEDE" IN TABLE 1

	x_{ak}	P_c	P_e	P_c—P_e
American	—2.0559	.020	.017	.003
Armenian	1.2546	.895	.891	.004
Chinaman	1.6951	.955	.945	.010
Englishman	—1.1076	.134	.122	.012
Frenchman	—0.3131	.377	.389	—.012
German	—0.2481	.402	.326	.076
Greek	1.2127	.887	.916	—.029
Hindu	1.7286	.958	.898	.060
Irishman	—0.5132	.304	.256	.048
Italian	0.5377	.705	.800	—.095
Japanese	1.4364	.925	.916	.009
Jew	0.7160	.763	.753	.010
Mexican	1.5511	.940	.941	—.001
Negro	2.0932	.982	.962	.020
Pole	1.0680	.857	.881	—.024
Russian	0.8435	.801	.799	.002
Scotchman	—0.5739	.283	.201	.082
Spaniard	0.6255	.734	.754	—.020
South American	0.5186	.698	.765	—.067
Swede				
Turk	2.0593	.980	.974	.006
	12.5285	13.600	13.506	.094

$(P_c—P_e) = .094$

$|P_c—P_e| = .590$

$$\text{Average discrepancy} = \left|\frac{P_c—P_e}{n}\right| = \frac{.590}{20} = .030$$

Check for x_{ak}: $nS_a(.707) — \Sigma(.707)S_k = \Sigma(.707\ S_a — .707\ S_k) = 12.5285$

Check for $P_c—P_e$: $\Sigma P_c — \Sigma P_e = \Sigma(P_c — P_e) = .094$

ties as in the previous tables. The second column gives the calculated value of

$$x_{ak} = \frac{S_a - S_k}{\sqrt{2}} = .707\ S_a - .707\ S_k\ ,$$

in which

x_{ak} = the sigma value of the calculated proportion of subjects who should prefer nationality "a" rather than nationality "k."

S_a = the scale value for "Swede."

S_k = the scale value of each of the other nationalities in turn. Consequently, the second column in Table 7 will be calculated by the formula

$$x_{ka} = .707 \times 2.9079 - .707\ S_k = 2.0559 - .707\ S_k.$$

The next column gives the calculated proportion, P_c corresponding to the calculated value of x_{ak}. It is obtained directly from the Kelley-Wood tables. Adjacent is the experimental proportion, P_e, obtained from Table 1. The discrepancy between the experimental and the calculated proportions is shown in the last column of Table 7. The average discrepancy for the whole column is calculated at the foot of the table. The last column of the table is inserted partly to enable one to inspect the discrepancy for each pair of nationalities and also for the purpose of arithmetical check on the calculations. The average discrepancy for the twenty nationalities compared with "Swede" is .03 which happens to be the same as the average discrepancy between Tables 1 and 8. The calculations in this study involved twenty-one tables like Table 7, one for each column of Table 1. The average discrepancy of .03 for the twenty comparisons with "Swede" is shown at the foot of the column "Swede" in Table 8. The last row of Table 8 shows the average discrepancy for the twenty comparisons with each nationality.

Inspection of the discrepancies for the different nationalities reveals that the highest discrepancy of .06 occurs for "Jew." Since 15% of the subjects were Jews and since the prejudice against them is common among other members of the group, it is quite probable that the distribution of attitude toward the Jews in this group of subjects deviates considerably from the normal probability curve. It might even be bimodal. Even if the distribution can be represented roughly as bell-shaped, its dispersion would probably be larger in this group than the dispersion for other nationalities. Either one of these factors would cause a noticeable discrepancy between the experimental and the calculated proportions because our procedure in Case V as-

TABLE 8
CALCULATED PROPORTIONS

	American	Armenian	Chinaman	Englishman	Frenchman	German	Greek	Hindu	Irishman	Italian	Japanese
American	—	.000	.000	.171	.041	.035	.000	.000	.061	.005	.000
Armenian	1.000	—	.330	.991	.942	.934	.517	.318	.961	.763	.428
Chinaman	1.000	.670	—	.997	.978	.974	.685	.487	.986	.876	.602
Englishman	.829	.009	.002	—	.214	.195	.010	.002	.276	.050	.005
Frenchman	.959	.058	.023	.787	—	.474	.063	.021	.579	.197	.040
German	.964	.067	.026	.805	.526	—	.072	.024	.605	.216	.046
Greek	1.000	.483	.315	.990	.936	.928	—	.303	.958	.750	.411
Hindu	1.000	.682	.513	.998	.979	.976	.697	—	.987	.883	.615
Irishman	.938	.039	.014	.724	.421	.395	.042	.013	—	.147	.026
Italian	.995	.237	.124	.950	.803	.784	.250	.117	.853	—	.184
Japanese	1.000	.572	.398	.995	.960	.954	.589	.385	.974	.816	—
Jew	.997	.295	.164	.966	.848	.832	.310	.156	.891	.571	.236
Mexican	1.000	.617	.443	.996	.969	.964	.632	.430	.980	.845	.546
Negro	1.000	.799	.655	1.000	.992	.990	.811	.642	.995	.940	.744
Pole	1.000	.426	.265	.985	.916	.906	.442	.254	.943	.702	.356
Russian	.998	.341	.197	.975	.876	.862	.356	.188	.913	.620	.277
Scotchman	.931	.034	.012	.703	.397	.372	.037	.011	.476	.133	.022
Spaniard	.996	.265	.143	.958	.826	.809	.279	.135	.873	.535	.209
South American	.995	.231	.120	.948	.797	.778	.244	.113	.849	.492	.179
Swede	.980	.105	.045	.866	.623	.598	.113	.042	.696	.295	.075
Turk	1.000	.790	.642	1.000	.991	.989	.801	.630	.995	.936	.733
Σp	19.582	6.720	4.431	17.805	15.035	14.749	6.950	4.271	15.851	10.772	5.734
$\lvert p_o - p_e \rvert$	.012	.025	.020	.026	.040	.035	.032	.027	.031	.044	.028

TABLE 8 (*continued*)

	Jew	Mexican	Negro	Pole	Russian	Scotchman	Spaniard	South American	Swede	Turk	Check Column
American	.003	.000	.000	.000	.001	.069	.004	.005	.020	.000	0.415
Armenian	.705	.383	.201	.574	.660	.966	.735	.769	.895	.210	13.282
Chinaman	.836	.557	.345	.735	.803	.988	.858	.880	.955	.358	15.570
Englishman	.034	.004	.000	.015	.026	.297	.042	.052	.134	.000	2.196
Frenchman	.152	.031	.008	.084	.124	.603	.174	.203	.377	.009	4.966
German	.167	.036	.010	.094	.138	.628	.191	.222	.402	.011	5.250
Greek	.690	.368	.189	.558	.644	.963	.721	.756	.887	.199	13.049
Hindu	.844	.570	.358	.746	.812	.989	.865	.887	.958	.370	15.729
Irishman	.110	.020	.005	.057	.087	.524	.127	.151	.304	.005	4.149
Italian	.429	.155	.060	.298	.380	.867	.465	.508	.705	.064	9.228
Japanese	.764	.454	.256	.644	.723	.978	.791	.821	.925	.267	14.266
Jew	—	.202	.084	.363	.449	.901	.536	.578	.763	.090	10.232
Mexican	.798	—	.294	.685	.760	.983	.823	.849	.940	.306	14.860
Negro	.916	.706	—	.847	.894	.996	.929	.942	.982	.514	17.294
Pole	.637	.315	.153	—	.589	.950	.671	.709	.857	.161	12.237
Russian	.551	.240	.106	.411	—	.922	.586	.627	.801	.112	10.959
Scotchman	.099	.017	.004	.050	.078	—	.115	.137	.283	.004	3.915
Spaniard	.464	.177	.071	.329	.414	.885	—	.543	.734	.076	9.721
South American	.422	.151	.058	.291	.373	.863	.457	—	.698	.062	9.121
Swede	.237	.060	.018	.143	.199	.717	.266	.302	—	.020	6.400
Turk	.910	.694	.486	.839	.888	.996	.924	.938	.980	—	17.162
Σp	9.768	5.140	2.706	7.763	9.042	16.085	10.280	10.879	13.600	2.838	210.001
$\lvert p_c - p_e \rvert$	.061	.031	.018	.028	.022	.029	.041	.027	.030	.021	.628

Average discrepancy $= \frac{.628}{21} = .0299$

sumes that the distribution of attitude toward each nationality is normal and that the dispersions are the same for all of the nationalities and races.

The average discrepancy between Tables 1 and 8 is shown in the last row of Table 8 to be .0299 or practically three per cent. It is very satisfactory considering the fact that the equation that has been used for the scaling contains a number of assumptions that are undoubtedly only roughly approximated by the data.

There were twenty-one repeated pairs in the original mimeographed forms on which the nationality preferences were recorded by the 239 subjects. These repetitions were so arranged that every nationality was represented twice in the twenty-one repeated pairs. The repetitions were scattered at random throughout the lists so that the subject would have slight opportunity to recall his first judgment on a particular pair if he was at all in doubt. The object of these extra or repeated pairs was to ascertain the average error of the original proportions themselves so that these might be compared with the magnitude of the discrepancies between the experimental and calculated proportions. The average difference between the two proportions of the repeated pairs was .019 or nearly two per cent. When this error of the original proportions is compared with the discrepancy between the experimental and calculated proportions which was .0299 or practically three per cent, it is seen that the psychophysical method here described fits the data with surprising accuracy. In fact, the average discrepancy of two per cent in the original experimental proportions accounts for a part of the discrepancy between the experimental and the calculated proportions.

Summary

1. The principal object of this study is to show that psychophysical formulations which are made ideally for discriminatory judgments of simple physical stimuli can be applied also to discriminatory judgments involving social values even when these values are loaded with prejudice or bias. The law of comparative judgment in its simplest form, Case V, is here applied to discriminatory judgments of preference for nationalities and races. The group of 239 subjects was intentionally heterogeneous in that 40% or more of the parents and grandparents were foreign-born, 15% were Jews, 64% were Protestants, and 10% were Catholics.

2. The method of paired comparison was used as the experimental procedure. A proportion of judgments of preference was determined for every possible pair of nationalities. On the basis of these experimentally observed proportions the scale value of each nationality or race was calculated by the law of comparative judgment. These are shown graphically in Figure 1.

3. The internal consistency of the calculations is shown by the discrepancies between the experimental and the calculated proportions. The average discrepancy between them was only three per cent. The average difference between the repeated proportions in the original form was two per cent.

4. The law of comparative judgment can be used for measuring objectively the degree of similarity between two groups as regards their national and racial attitudes, prejudice, or bias. This is accomplished by correlating the scale values of the same list of nationalities for both groups. If the correlation coefficient is high, the two groups are similar in their attitudes toward nationalities and races. If the coefficient is low, they are different in their national and racial preferences.

5. Another application of the law of comparative judgment is in the quantitative measurement of the degree of tolerance of a group for the nationalities in the list. Any measure of the scatter of scale values for the whole list can be used as a quantitative index of the tolerance of the group. When the scatter is wide, the tolerance is low. When the scatter is small, the group is correspondingly more tolerant.

6. The law of comparative judgment can be used to describe objectively the attitudes of a group of subjects toward nationalities and races. The same method can undoubtedly be used also for describing the attitudes of a group toward other social values which are saturated with prejudice and bias. The method is free from the effects of the personal bias of the individual investigator. It is hoped that psychophysical tools may find application not only in the restricted field of sensory discrimination but also in the study of other psychological and social values.

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UNE ÉTUDE EXPÉRIMENTALE DES PRÉFÉRENCES DE NATIONALITÉ

(Résumé)

Cette étude expérimentale est une application des méthodes psycho-physiques à la mesure des attitudes sociales. Dans la vérification de la loi de Fechner, par exemple, nous avons deux variables, c'est-à-dire; (1) la grandeur du stimulus physique, l'échelle *R*, et (2) le continuum psychlogique, l'échelle *S*. Dans la construction des échelles psychologiques et éducationnelles nous n'avons pas d'échelle *R* mais nous classons des spécimens, des noms, ou d'autres stimuli selon un continuum ou échelle psychologique. Au lieu de demander à un sujet de juger lequel de deux poids est le plus lourd, comme dans les expériences psycho-physiques ordinaires, nous demandons maintenant au sujet de dire laquelle de deux nationalités il aimerait mieux fréquenter. Par l'application de la loi de jugement comparatif à ces données, nous construisons une échelle *S* définie par les classements de vingt

nationalités et races. On a fait l'expérience selon la méthode de comparaisons en paires.

Les valeurs des vingt nationalités sur l'échelle indiquent les attitudes sociales du groupe expérimental envers ces nationalités. Cette méthode expérimentale donne trois indices quantitatifs des attitude sociales: (1) On peut mesurer en termes de l'erreur discriminative le degré de discrimination faite entre deux nationalités par un groupe d'individus comme groupe. (2) Le degré de similarité dans les attitudes internationales de deux groupes peut être constaté quantitativement comme une corrélation entre les échelles individuelles construites pour les deux groupes selon leurs jugements de la même liste de nationalités. (3) Le degré de tolérance d'un groupe pour une liste de nationalités peut être comparé quantitativement avec la tolérance d'un autre groupe pour la même liste de nationalités. Une mesure quelconque de dispersion des valeurs de l'échelle constitue un indice quantitatif de la tolérance de ce groupe pour la liste donnée de nationalités.

On montre qu'une valeur de l'échelle pour chacune des vingt nationalités suffit pour expliquer les 200 proportions expérimentales avec une très petite erreur de mesure.

THURSTONE

EINE EXPERIMENTELLE UNTERSUCHUNG DER BEVORZUGUNG VERSCHIEDNER NATIONALITÄTEN

(Referat)

Dieses Experiment ist eine Anwendung der psychophysikalischen Methoden zur Bemessung sozialer Einstellung. In der Bewahrheitung von Fechner's Gesetz für das Heben verschiedner Gewichte, z. B. haben wir zwei Varianten, nämlich; (1) die Stärke des physikalischen Reizes, die *R*-Skala, und (2) das psychologische Continuum, die *S*-Skala. In der Konstruction der psychologischen und Bildungsskala haben wir keine *R*-Skala, aber wir stellen Exemplare, Namen, oder andere Reizmittel an einem psychologischen Kontinuum oder an einer Skala auf. Anstatt die Versuchsperson entscheiden zu lassen, welches von zwei Gewichten das Schwerere ist, wie in den klassischen psychophysikalischen Experimenten, fragen wir nun die Vp. mit welcher von zwei gewissen Nationalitäten sie lieber verkehren würde. Durch Anwendung des Gesetzes des vergleichenden Urteils an diese Daten, konstruieren wir nun eine *S*-Skala, welche durch die Verteilung von 20 Nationalitäten und Racen bestimmt wird. Das Experiment wurde nach der paarweisen Vergleichungsmethode ausgeführt.

Das Ergebnis dieses Experiments, die Skalawerte der 20 Nationalitäten, deutet die soziale Einstellung der Experimentgruppe in Bezug auf diese Nationalitäten an. Als Resultat des Experiments haben wir drei quantitative Ergebnisse: (1) Der Grad, bis zu dem eine Gruppe von Individuen, als Gruppe, zwischen irgendwelchen zwei Nationalitäten unterscheidet, kann in Werten des Unterscheidungsfehlers bemessen werden. (2) Der Grad der Ähnlichkeit in der internationalen Einstellung zweier Gruppen, kann, als eine Korrelation zwischen den einzelnen Skalen quantitativ ausgedrückt werden, welche für diese zwei Gruppen aufgestellt wurden, auf Grund ihres Urteils über dieselbe Liste von Nationalitäten. (3) Der Grad, der Toleranz einer Gruppe in Bezug auf eine Liste von Nationalitäten, kann man mit der Toleranz einer andern Gruppe gegen dieselbe Liste von Nationalitäten quantitativ vergleichen. Irgendwelche Zerteilung der Skalawerte ist ein quantatitiver Index der Toleranz dieser Gruppe gegen die betreffende Liste von Nationalitäten.

Es wird bewiesen dass ein Skalawert für jede der 20 Nationalitäten

genügt, um die 200 experimentellen Proportionen, mit einem sehr geringen Messungsfehler, zu erklären.

THURSTONE

ЭКСПЕРИМЕНТАЛЬНОЕ ИЗУЧЕНИЕ ПРЕДПОЧТЕНИЙ НАЦИОНАЛЬНОСТЯМ

(Реферат).

Настоящий эксперимент заключается в применении психофизиологических методов к измерению общественных предрасположений. При проверке закона Фехнера для поднятых тяжестей мы имеем дело с двумя переменными, а именно: (1) с величинами физических стимулов R-шкалою, и (2) с психологической непрерывностью, S-шкалою. При построении психологических и педагогических шкал у нас нет S-шкалы, но мы расщеделяем образчики, имена, или другие стимулы вдоль психологической непрерывности, или шкалы. Вместо того, чтобы спрашивать испытуемого, которая из двух тяжестей тяжелее, как в классических психофизиологических опытах — мы предлагаем ему вопрос, с которой из двух национальностей он предпочел бы знаться. Посредством применения закона сравнительного суждения мы конструируем S-шкалу, на основании распределений двадцати национальностей и рас. Эксперимент был произведен посредством метода «парочных сравнений». Полученные значения из шкалы с двадцатью национальностями показывают общественные предрасположения экспериментальной группы к этим национальностям. Эксперимент дает три количественных показателей общественных предрасположений: (1) Размер различения группой индивидов, как таковой, между двумя любыми национальностями может измеряться посредством ошибки различения. (2) Степень сходства двух групп в интернациональных предрасположениях может быть количественно выражена корреляцией между двумя отдельными шкалами, построенными на основании суждений этих двух групп об одном и том же списке национальностей. (3) Степень толерантности одной группы по отношению к какому-нибудь списку может быть количественно сравниваемо с толерантностью другой группы к тому-же списку. Любая мера дисперссии является количественным показателем толерантности группы к данному списку национальностей. Показано, что одно значение шкалы достаточно для 200 экспериментальных пропорций с весьма малой ошибкой измерения.

ТЕРСТОН
(Thurstone)

THE MECHANISM OF TONIC IMMOBILITY ("ANIMAL HYPNOSIS")*

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I

It may be said that in general an animal can react in one of two ways to stimuli which tend to influence its behavior. The animal may respond positively to the stimulating object by making appropriate motor adjustments, such as of attack or retreat, or manipulative movements; or it may react in a second way which has been surprisingly overlooked by most students of animal behavior: it may cease all movement and remain quiescent even in the presence of violently disturbing factors. This phenomenon is strikingly demonstrated by a great variety of forms ranging from planarians to man. It has been variously described as "animal hypnosis," "cataplexy," and "playing possum." Some writers (Holmes, 1908) have spoken of it as an "instinct of death feigning" because of its apparent survival value. The best label is probably *tonic immobility* (Crozier, 1923), since by this expression one defines the occurrence in terms of characteristic physiological criteria.

Tonic immobility is conspicuously demonstrated in comparatively sessile animals such as echinoderms and coelenterates which have limited means of responding to environmental disturbances. Sea anemones are examples of this group, noxious stimuli causing withdrawal of the tentacles and contraction of the body musculature (Parker, 1922). Among many animals protected by hard or spiny coverings, such as certain molluscs, arthropods, and vertebrates, the closing of the shell, the retraction of appendages within the shell, and the rolling into protective attitudes may be basically phenomena of tonic immobility.

The state is seen in its most striking form in many arthropods, which become immobile in characteristic positions when tactually stimulated in certain ways. Rabaud (1919) discusses at length the condition as seen in these animals. He points out that most arthropods have regions of the body, serving as loci of reflexes, which when tactually stimulated, cause the animal to pass into a state of immobility characterized by pronounced tonicity of the skeletal muscula-

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ture. The durations of the periods of immobility vary considerably from species to species and to a lesser degree with individuals within the species. The antennae, thorax, and base of the wings are, in general, regions of the insect body which when stimulated, tend to produce immobilization, while other parts of the body, most particularly the feet, seem to be the seat of antagonistic reflexes causing resumption of activity. With some arthropods the mere lifting of the animal from the substratum produces immobilization; this may be caused by the sudden release of reflex tension in the limb muscles. The state is conspicuous by the fact of its apparent all-or-none nature and because of the characteristic positions assumed by the animals.

Tonic immobility among lower vertebrates was systematically discussed in a paper by Verworn in 1898. Mangold in 1920 described the condition in a variety of higher vertebrates. With birds and mammals the state is usually obtained by turning the animal suddenly into a dorsal position. Guinea pigs are more readily immobilized by this method than most mammals, although the condition has been produced in young dogs, cats, apes, sheep, and men. Among birds the state is well demonstrated in hens and pigeons. As early as 1562 the Abbé Kircher records the phenomenon in a hen (cf. Piéron, 1913; Mangold, 1920). Foxes and opossums are said to "feign death" sometimes when pursued by enemies and to remain quite immobile even if mauled about. In such cases it has been urged that the condition is a "death feint" of definite survival value, and as a result it has been spoken of as an "instinct of death feigning."

Tonic immobility or a state akin to it has been described in children by Piéron (1913). I have recently been able to produce the condition in adult human beings. The technique was brought to my attention by a student in physiology, Mr. W. I. Gregg, who, after hearing a lecture on tonic immobility, suggested that a state produced by the following form of man-handling, which he had seen exhibited as a sort of trick, might be essentially the same thing. If one bends forward from the waist through an angle of 90°, places the hands on the abdomen and after taking a deep breath, is thrown violently backwards through 180° by a man on either side, the skeletal muscles contract vigorously and a state of pronounced immobility lasting for some seconds may result. The condition is striking and of especial interest since this type of manipulation (sudden turning into a dorsal position) is the most common one used for producing tonic immobility in vertebrates.

In all cases from coelenterates to mammals the condition is marked by a high degree of plastic tonus of skeletal musculature and a lack of reactivity to environmental disturbances. Verworn (1898) speaks of it as animal hypnosis because of the similarity of the plastic tonus to that shown in the cataleptic trance of the human hypnotic subject.

The term, however, is not a very happy one in this connection because of its anthropomorphic implications.

Work by Rabaud (1919) and also by Holmes (1906) has tended to show that in arthropods the state is not dependent on any one neurone center. On destroying various ganglia the condition has been shown to continue. *Ranatra,* the "water scorpion," even when cut in two continues the "feint" for its customary period (Holmes, 1906).

Certain stigmata of neuropathology may involve essentially the same condition. The katatonic state of dementia praecox patients bears many points of at least superficial resemblance to tonic immobility. Also the phenomena of contracture and catalepsy accompanying functional disorders caused by violent psychological disturbances, such as have been illustrated by certain cases of war neuroses, resemble tonic immobility to some extent.[1]

From what has been said it may be seen that tonic immobility is actually a special case of that important physiological problem which is concerned with the mechanisms underlying alternate periods of movement and quiescence on the part of organisms. Pavlov ("Conditioned Reflexes" [1927] page 21) says that little . . . "has been done towards the elucidation of the class of negative or inhibitory reflexes (instincts) which are often evoked by any strong stimulus or even by weak stimuli if unusual. Animal hypnosis so-called, belongs to this category." In 1923 Crozier suggested that the study of tonic immobility might serve as an avenue of approach to complicated central nervous processes and shed light on the problems of movement and non-movement, inhibition, and the mechanism of the synapse. In consequence of these suggestions a series of experiments was carried out. These experiments will be discussed in later sections of this paper.

II

Before proceeding to the question of the physiology of tonic immobility, the general aspects of the problem may be better understood by a brief discussion of its possible phylogenetic significance. The notion that it is a true "death feint" had wide vogue during the past century. Charles Darwin, however, showed that the postures of "feigning" animals as a rule do not resemble those of the *dead* animal.

[1]Verworn (1898) suggests that "animal hypnosis" results from excitement of the animal which disturbs the functional adjustment of higher centers, inhibiting them and allowing a stream of impulses to pass from lower centers of reflex tonus to the muscles. He compares the tonic condition of the muscles to that found in decerebrate rigidity. He also compares immobility during "animal hypnosis" to the inactivity in man resulting from highly exciting situations.

He compared the postures of a large number of arthropods in tonic immobility with those of animals of the same species "naturally" dead and also with those of some killed by asphyxiation. In only a few cases were the postures similar, the dead animal and the "feigning" animal usually assuming totally different positions of the appendages. This has been confirmed by later observers.

Rabaud in his paper of 1919 argues that in all probability tonic immobility has no survival value. He points out that while some arthropods curl up in protective attitudes, others do not, and that frequently the most active and vigorous insects are the best feigners—animals that one would expect might easily escape danger by flight. On the other hand, he notes the great camouflage value of immobility; but, since most arthropods are immobile a great part of the time anyway, an elaborate mechanism for rendering them helpless in the face of danger seems to him hardly necessary. Survival value based on this argument further requires the doubtful assumption that vision is the principal sense employed by preying insects. Rabaud also cites observed instances of insects captured by carnivorous enemies, which were devoured in spite of the fact that they became immobilized when attacked.

From these considerations it might appear that tonic immobility is a "behavior pattern" or, more precisely, a mode of conduct dependent upon a general feature of nervous organization, and that no special mechanism of survival value has been developed to produce the effect. However, it is difficult to see how so common and important a function could develop in animals with vastly different anatomical organizations if it had no phylogenetic significance. It seems to the writer that the importance of tonic immobility as a form of "camouflage" may have been greatly underestimated by Rabaud and other writers on the subject who have failed to consider the following facts.

The reactivity of animals to objects, which we may loosely infer is a measure of the "perception" of the object by the animal, is an interesting problem in configurationism. Köhler's work with apes has tended to show that the ability to react to object situations as such is at a much lower level in chimpanzees than it is in man. The "grouping together" of configurational patterns appears to be one of the most complex of mental functions. As one descends the vertebrate scale it seems quite probable that the reactivity to object situations approaches a very primitive level indeed.

In the case of man it is a fact of common experience that movement of an object with respect to its environment greatly facilitates the perception of the object. This is especially true of objects on the peripheral field of the retina which seems to be more sensitive to relative motion than are the central regions (cf. Piéron, 1926). It

is also noteworthy that stationary objects are very poorly perceived on the peripheral retina as contrasted to the central region. Anatomically, this may possibly be correlated with the fact that the peripheral retina is composed principally of rod receptors while as one approaches the fovea the density of cones becomes progressively greater. Among lower vertebrates, rod retinae are common while cone retinae seem to be more recently developed organs, frequently being quite absent among fishes, amphibians, reptiles, birds, and certain of the more primitive mammals. From these facts one might possibly expect that animals with rod retinae would be comparatively reactive to moving objects and less reactive to stationary objects, and that the safety of their prey would be greatly augmented as long as the prey remained immobile.[2]

Aside from this parallelism between the rod receptors of lower vertebrates and the rods of the human retina, the problem of central integration of the perceptive function is suggestive. In the case of man it is well known that perception depends upon a complicated functional interrelation of highly developed cortical areas. Lesions in the parietal lobes for example, may cause failure of the ability to perceive and recognize familiar objects even though the occipital region and optic tracts are intact. Many clinical cases of this sort are on record (cf. Piéron, 1926) and need not be described here. In some of these cases in which the actual visual sensitivity to light is good, objects can be perceived only if they move with respect to the environment. These facts are interesting in the present connection because among lower vertebrates in which the cortex is comparatively simple and underdeveloped, one might expect that the reactivity to objects would be at a very elementary level, perhaps in some cases determined only by that most fundamental of *perceptive* criteria—relative motion.

To test these suggestions I studied the feeding habits of certain lizards. *Phrynosoma strumosa,* one of the so-called "horned toads," were fed live meal-worms (*Tenebrio larvae*), by dropping them into the cage near the animals. Under these circumstances the worm was sometimes seized immediately and devoured, a situation occurring almost invariably if the worm started to creep after it had been dropped into the cage. If, however, as usually happened, the worm became immobilized by the shock of the fall, it was comparatively safe even though surrounded by *Phrynosoma,* until it recovered ac-

[2]Yerkes (*Harvard Psychological Studies,* 1) notes that frogs apparently never feed on motionless objects but seize any small moving object within reach. This is also characteristic of certain fishes and amphibians. A study by Waugh (1910) shows that the mouse scarcely reacts at all to stationary objects. He also notes that the retina of the mouse is without cones. Yerkes (1910) observed that kittens relatively inexperienced in the killing of mice do not react to mice as long as they remain stationary.

tivity. The lizards apparently failed to "see" the quarry as long as it was motionless. They would often gather in a circle around the spot where the larva had fallen, having apparently reacted to the motion of the falling body, but did not touch it until it moved, thus giving one, in the meantime, the impression of "failing to find something they were looking for." These same lizards apparently even have a method of creating relative motion artificially, which is employed when exploring unfamiliar territory or when searching for an immobile worm that may have been dropped at their feet. While standing motionless they will frequently raise and lower the head vigorously a number of times, thereby causing a relatively displacement of images on the retinae. This reaction which seems to be of a reflex nature has sometimes, though rarely, enabled a lizard to find the immobile worm. From the foregoing evidence it seems probable that the "camouflage" value of tonic immobility may be significant and out of all proportion to what might be expected from anthropomorphic considerations of normal human perception.

Prior to 1923 the only attempts to investigate the physiological mechanism of tonic immobility were the experiments of Holmes and Rabaud already mentioned, in which various nerve ganglia in arthropods were cut and the resulting effect on the nature and duration of subsequent periods was observed. This work tended to emphasize the diffuse neurological basis of the phenomenon and its apparent independence of any one neurone center. In 1923 Crozier and Federighi investigated the condition in the arthropod *Cylisticus convexus* with a view of ascertaining its physiological foundation. The method employed for this purpose was ingenious, and may turn out to be highly significant for the study of animal behavior. For this reason it will be well to digress radically at this point and describe it in some detail.

III

Events occurring in living systems are known to be markedly affected by temperature. The range of temperature over which life can exist is small. To assure optimum control of this important variable the higher vertebrates have developed complicated neurological mechanisms for maintenance of constant body temperature. Among these animals, with the exception of those that hibernate, fluctuations of a few degrees may produce irreversible chemical changes which result in death.

The effect of temperature on chemical reactions has long been a matter of interest. The relation between temperature and equilibrium points, rates of reactions, and the nature of resulting end-products has been examined by chemists in detail in numerous homo-

geneous and heterogeneous inorganic and organic systems. In this work temperature has proved to be an especially convenient physical variable because of its comparative ease of control and the high degree of accuracy of the methods available for its measurement.

The influence of temperature on rates of reaction is very marked, the rates being usually doubled and sometimes trebled for a rise of temperature of 10°C. at ordinary temperatures. In the case of reactions in solution, which are the most important ones from the standpoint of biochemistry, other variable factors besides the temperature, which in different cases may determine the rates of chemical change, are the amount of undecomposed reactants, the amount of products formed by the reaction, diffusion effects, the surface distributions of catalyst and substrate, and the concentration of the dissolved catalyst.[3]

Let us consider the simplest possible case in which a single substance is undergoing chemical decomposition as follows:

$$A \rightarrow B + C + D + \ldots + N$$

where A is the reactant or substrate and B, C, D, and N are products of the decomposition.[4] Assuming the temperature and amount of catalyst to be constant one may write

$$\frac{dA}{dt} = -k\ (A_o - x)$$

where A_o is the amount of A present at t_o, the beginning of the reaction, and x is the amount of A that has decomposed at time t; x is directly measurable by analysis of the products of decomposition. In this equation k is called the *velocity constant* and is an important characteristic of the reaction when extraneous variables are properly controlled. In the case of more complicated multimolecular processes, where instead of one substance (A) decomposing there may be several reacting together, the dynamical formulation is more complicated but the velocity constant is defined in the same general way.

A crude empirical formulation of the effect of temperature on chemical processes is expressed by the ratio of the velocity constant of a reaction at a given temperature, T, and the constant at a tem-

[3] A catalyst is a substance which accelerates a reaction but which is found at the conclusion of the reaction to be in the same form and in the same concentration as at the start. It is probable that in solutions all chemical reactions with measurable velocities are catalyzed. Very minute traces of catalyst are usually effective.

[4] It is extremely doubtful if reactions as simple as this actually occur. It is probable that the catalyst in promoting the reaction forms intermediate compounds with the substrate. However, for practical purposes this paradigm is useful in describing a so-called monomolecular reaction and is the formulation of a "first order" process.

perature ten degrees lower, $(T - 10°)$. This ratio, $\frac{K_T}{K_{T-10}}$ is called the "temperature coefficient" and is written symbolically as Q_{10}. For practical purposes it serves as a rough index only, but has no theoretical significance. The value of Q_{10} varies between two and three for most chemical processes (at ordinary temperatures). The corresponding temperature effect for a physically controlled process such as diffusion gives a value of Q_{10} of approximately unity.

A better formulation of the effect of temperature is that expressed by an empirically derived equation proposed by Arrhenius in 1889. This equation has been shown to have very wide application and in recent years it has been given theoretical interpretation by Tolman (1927), Marcelin, J. Rice, and G. N. Lewis. The equation may be written in several ways, but the most convenient is probably the exponential form

$$\frac{k_2}{k_1} = e^{\frac{\mu}{R}\left(\frac{1}{T_1} - \frac{1}{T_2}\right)},$$

where k_1 and k_2 are the velocity constants at the absolute temperatures (Kelvin scale) T_1 and T_2; e is the base of natural logarithms, 2.718+; R is the well-known gas constant; and μ, called by Crozier the "temperature characteristic" (Crozier, 1924*b*), is a constant with the dimensions of calories per gram molecule of reacting substance. The constant μ is important because its magnitude has been shown to depend upon the nature of the specific catalyst controlling the reaction. If μ can be calculated from the data it may serve as an index of the controlling catalyst present in quantities so minute as to render direct chemical analysis impossible.[5]

Not only has μ a definite practical value for analytical purposes but it also has theoretical significance. In recent years the Arrhenius equation has been derived mathematically from purely theoretical considerations of chemical dynamics and statistical mechanics. μ is now regarded as the number of energy units that must be absorbed by a gram molecule of reactant (in this case, catalyst) to raise its energy content from the inactive to the active state. According to

[5]It should be cautioned that the same value of μ is not necessarily always found for the same physiological function. That is to say, for example, there is no particular μ universally characteristic of heart rate alone. There are, however, certain values which hold for heart rates fairly constantly among related species of animals. This in no way invalidates the theoretical significance of the temperature characteristic for physiological analysis (cf. Crozier and Stier, 1927).

this theory, for catalysed reactions the catalyst is assumed to form a temporary intermediate compound with the substrate which accelerates the reaction, the intermediate compound decomposing and resupplying the catalyst in its original form as the main reaction proceeds (Tolman, 1927).

It follows from the nature of the Arrhenius equation that when one plots against the reciprocals of the corresponding absolute temperatures, the experimentally determined values of the logarithms of the velocity constants of a chemical reaction at different temperatures, a linear function is obtained, from the slope of which the value of μ for the process may be calculated. Characteristic values of μ have been obtained for reactions involving numerous substances in solution. In such cases the number of values of μ found is surprisingly limited, certain values occurring again and again with widely differing substrates. Since these values may correspond to the nature of controlling catalysts, the same μ occurs for a diversity of processes catalysed by the same substance. For example, in case of reactions known to be catalysed by hydrogen ion the value of μ is 20,000 calories. For reactions catalysed by hydroxyl ion a value of 11,300 calories is obtained; while for the large group of biochemical oxidative reactions known to be catalysed by iron the value of μ is 16,200 calories. Other examples might be cited, but these are sufficient to indicate the significance of the constant in question.

IV

In the last four years the effect of temperature on biological processes has been carefully examined by W. J. Crozier. Prior to 1923 much work of this kind had been done but for the most part the results had been expressed in terms of the more or less crude Q_{10} ratio which is only a very approximate empirical generalization with little theoretical significance. It occurred to Crozier that certain rhythmical physiological processes, such as heart rate and respiratory movements, might be controlled directly by simple chemical mechanisms in such a way that their frequency might turn out to be proportional in an exact way to the underlying velocity constants of the chemical reactions. If this were true it might, therefore, be possible to obtain knowledge of the actual catalysts controlling the chemical processes underlying physiological events. The importance of this approach is obvious if it be borne in mind that the catalysts in question are usually present in such minute traces and in such inaccessible regions of the living organism as to render direct analysis by any known chemical means quite impossible. With this hypothesis in mind, he examined the effect of temperature on a large variety of biological functions, using, for the most part, small intact poikilo-

thermous organisms placed in very accurate thermostats. The data so obtained were correlated through the Arrhenius equation by plotting the logarithms of the frequencies of the movements, assumed proportional to the logarithm of the velocity constant, against the reciprocals of the corresponding absolute temperatures. In the very great majority of these cases surprisingly good linear functions were obtained, indicating a chemical mechanism of control. Moreover, what is still more surprising, certain definite values of μ were found to occur with great accuracy again and again for a most diverse variety of physiological events.

These μ values are in magnitude identical with those of general chemistry, the values of 20,000 for hydrogen ion, 11,300 for hydroxyl ion and 16,200 for iron catalysis being among the most common.

In addition to numerous experiments by Crozier and his collaborators, which for the most part have been published in the *Journal of General Physiology* since 1923,[6] Crozier has calculated the value of μ for miscellaneous observations of the effect of temperature on vital processes occurring in biological literature. These observations had been made by many workers for very diverse purposes, but where the observations appear reliable the characteristic constants are obtained ranging at definite intervals between 8,000 and 32,000 calories.

In addition to heart rates and frequencies of respiratory movements of animals the effect of temperature has been studied on such diverse phenomena as growth, the beating of cilia, the rate of creeping of insects, the rate of segmentation of eggs, the reaction times of organisms, and the production of CO_2 and utilization of O_2 by various tissues. In these cases the form of temperature frequency function and the values of μ obtained indicate a chemical mechanism controlled by specific catalysts which in some cases it has been possible to isolate and identify. In the case of respiratory oxidations for example, the work of Warburg, Meyerhof, and others has shown that small traces of iron are necessary for respiration; in all such cases a μ of 16,300 calories is obtained, occasionally associated with a value of 11,000 calories or of 8,000 calories. This last complexity, which cannot be further discussed here, in no way invalidates the general analysis (Crozier, 1924*d*, 1926) but merely reflects the unquestioned complexity of living matter.

An interesting example of the use of the temperature method in a somewhat different way is in a study by Crozier and Federighi (1924 *a*) of the effect of light in causing the slug *Limax* with one eye removed to creep in a circle. They were able to analyze the

[6] Cf. Crozier, 1924*b*, 1924*c*, 1924*d*.

mechanisms underlying these "circus movements" in such a way as to show that the variability of the behavior at room temperatures was the resultant of a conflict between two definite opposed physiological processes nearly balancing each other in effect at 15°C., but respectively "asserting their individuality" to a predictable degree on either side of this critical temperature.

It should be noted that temperature analysis offers a means of bridging the supposed gulf between the behavior of organisms and the processes of chemistry and physics. The Arrhenius equation not only correlates the empirical data but, because of its theoretical significance, it does so in a way which, in contrast to such generalization as the Weber-Fechner law, "makes sense" in terms of physical theory.

V

The first application of such temperature analysis to animal behavior was the previously mentioned investigation of tonic immobility in the land isopod *Cylisticus convexus* (Crozier, 1923; Crozier and Federighi, 1923). Holmes (1906) had found that arthropods, if successively reimmobilized as soon as they recovered activity, remained in the tonic condition for at first longer and then for shorter and shorter periods until they finally failed to be reimmobilized at all. From this Holmes concluded that the animals learned to refrain from the futile death feint. Crozier and Federighi, however, found by timing the durations of repeated immobilizations with a stop-watch, that the successive periods for arthropods definitely lengthen first to a maximum, and that when the process of successive immobilization was continued the durations progressively decreased again to a minimum, ultimately passing to a second maximum, and thus performing a definite cycle. They further discovered with continuation of reimmobilizations after spontaneous recovery that this cycle was repeated over and over again. Holmes's evidence for "learning" on the part of the animal apparently was due to the fact that he had worked with only a part of one of these cycles. They found that when the durations of the cycles were plotted against the temperature according to the Arrhenius equation

$$\frac{k_2}{k_1} = e^{\frac{\mu}{R}\left(\frac{1}{T_1} - \frac{1}{T_2}\right)},$$

good linear graphs were obtained. These functions gave two values of the constant μ, the critical thermal increment or *temperature characteristic*, of the orders of magnitude typical for other chemically controlled biological processes.

It was also observed that the initial immobilizing stimulus in a

series seems to start a chain of events which proceeds to a predetermined close even though this rhythm may be interrupted by premature forced "awakening" of the animal. In such cases the animal, if reimmobilized by the usual method[7] of gentle pressure on the thorax, continues to remain immobile until the time when it would have spontaneously recovered had the period not been interrupted. From these and other considerations,[8] Crozier suggested that the duration of immobility might be considered as proportional to the amount of a substance which is an intermediate product in a chain of reactions and which is responsible for maintenance of tonic contraction of the skeletal muscles.[9] This seemed to offer a means of approaching selective inhibition and integrative nervous action, and Crozier suggested that it might be extended to the consideration of the question of "synaptic resistance" in vertebrates.

VI

The problem has been pursued by the writer in the vertebrate field by an investigation of the mechanism of tonic immobility in the lizards *Anolis carolinensis* and *Phrynosoma strumosa*. An automatic mechanical method was developed for controlling extraneous variables connected with arousal and excitation, of regulating the temperature, and of recording periods of immobility, and a large number of kymograph records, containing 18,000 to 20,000 immobilizations were obtained for analysis. For temperatures above that of the room the animals, with the necessary apparatus, were enclosed

[7]Tonic immobility seems to be essentially of an all-or-none character and its duration is quite independent of the intensity of the stimulus used in producing it. Arthropods seem to have fairly definite regions which when stimulated produce the phenomenon. With vertebrates it may be produced readily by sudden turning into a dorsal position, by pressure on the thorax, or in some cases by gentle stroking between the eyes.

[8]Detailed descriptions of these experiments and their theoretical significance is soon to be published in the *Journal of General Physiology*.

[9]In the Arrhenius equation plot Crozier obtained two straight lines of different slope meeting at a critical temperature of 16°. Subsequent investigations have shown many functions of this general kind in biological processes. This may be explained by assuming that the substance controlling the duration of the process, in this case the duration of tonic immobility, is the mid-product, B, in a set of reactions as follows: $A \xrightarrow{k_1} B \xrightarrow{k_2} C$. In such a system B rises to a maximum from the reaction $A \rightarrow B$ and then falls to a minimum as the reaction $B \rightarrow C$ gets under way and consumes B. In such a system of reactions the slower of the two determines the value of μ. Since the reaction constants k_1 and k_2 are different and change at different rates with temperature, it follows that there will be a critical temperature above which k_1 is larger than k_2, or vice versa, and below which this order of magnitude is reversed, thus giving two lines of different slopes meeting at the critical temperature and yielding different values of μ.

in an air thermostat. For lower temperatures a very accurate thermostat was employed consisting of a water bath cooled by coils in series with an SO_2 condenser pump, which is operated by a relay system in series with a mercury contact switch immersed in the bath (Crozier and Stier, 1927*b*). The chamber containing the animals was immersed in the water bath.

The mechanism of the tonic immobility was found to bear many points of basic similarity to that suggested by Crozier for arthropods, but certain striking differences became apparent (Figure 1). The data of most of the records of durations of successive immobilizations were found to be fitted roughly by the Arrhenius equation giving evidence of chemical control of the durations of tonic immobility. Two characteristic μ values were obtained, one of which agrees closely with that obtained by Crozier with arthropods. However, in the case of *Anolis* there is no direct evidence for a set of catenary controlling reactions; the lines do not meet in the way characteristic for catenary processes.[10] There seem rather to be *two independent reactions.* One of these is evident over the range 5° to 35°C., with $\mu = 31{,}000\pm$ calories. The other, which acts simultaneously but independently, i. e., is superposed, as it were, on the first rhythm (see Figure 1), is effective between 5° and 20°C., and yields $\mu = 9{,}000\pm$ calories. These critical thermal increments are associated with certain physiological oxidation processes, but the corresponding specific catalysts in this case are not known. The data were taken over a five months' period, and the final increments are averages from numerous experiments with six animals. This is sufficient to account for a rather wide latitude of variation found in the results, since in most determinations of thermal increments μ is calculated from data taken with one animal in a short period, usually not greater than 24 hours; this procedure was of course impossible in the present case.

As with arthropods, forced recovery of the animal in the midst of a "period" has no effect on that period or on subsequent periods. The reimmobilized animal continues the "feint" until the time for its normal recovery.

To account for these results, the existence of two autacoids called α and β was originally assumed. It was suggested (Hoagland, 1927) that these substances were excreted in response to the immobilizing stimulus. In the case of *Anolis* immobilization usually resulted after a struggle, and it seemed probable that the autacoid might be excreted from an endocrine organ during the struggle.

To explain the chemical nature of the process controlling the nor-

[10]The temperature analysis was not carried our for *Phrynosoma.* It was found, qualitatively, that the general distribution of recovery periods resembles that with *Anolis.*

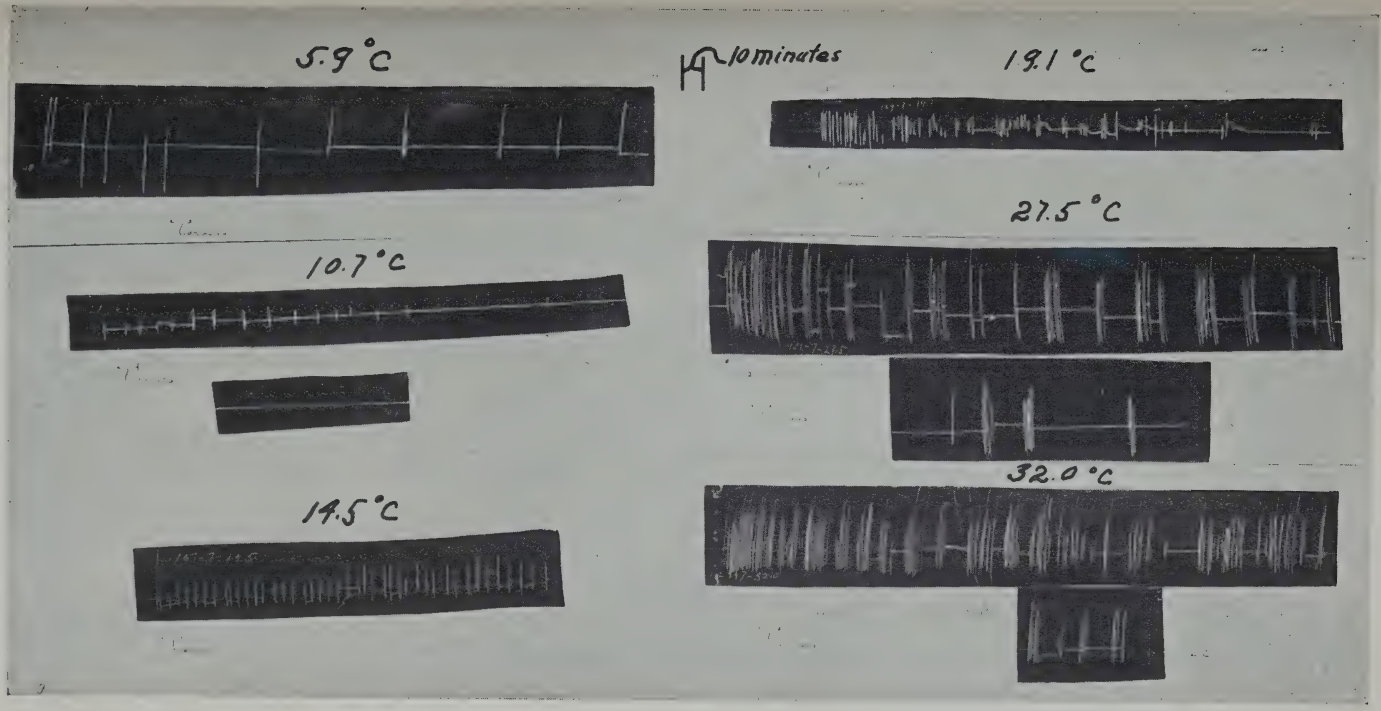

FIGURE 1

VERTICAL STROKES ON RECORDS REPRESENTING RECOVERIES AND REIMMOBILIZATIONS OF *Anolis*

The height of the vertical stroke has no significance, but the distance between the strokes is a measure of the time between spontaneous emergence from immobility. It will be noted that the periods decrease with rise of temperature. At approximately 20°C. a new rhythm seems to be superposed on the original one, becoming most conspicuous at 27.5°C. This rhythm in turn shortens at higher temperatures. These are samples from approximately 200 records.

mal durations, the autacoids were conceived as decomposing to active chalones (inhibitory hormones) A and B. The latter were regarded as favoring the passages of impulses from the tonic centers, at the same time inhibiting the activity of higher centers. Accordingly, the condition of tonic immobility would prevail as long as one of these substances was present above a certain threshold concentration. This threshold was qualitatively suggested by the rhythmic nature of the kymograph records (Figure 1) and by the sharpness of the spontaneous arousal of the animal on recovery; the animal passes suddenly from inactivity to violent movement.

In certain of the kymograph records, not shown in Figure 1, there is evidence of an accumulation of the assumed substance responsible for the maintenance of tonic contracture. A quantitative analysis of this effect shows that it may well be due to a limitation of diffusion of substance controlling the reaction since in terms of the basic assumptions a diffusion curve describes the relationship in these cases. Moreover, the very small magnitude of the temperature effect, $Q_{10} = 1.0$ in these cases is indicative of a physical rather than of a chemical process.

I have recently been carrying on experiments to ascertain the specific nature of the hypothetical hormones which might thus be involved. The injection of small amounts of adrenalin, in excess of a definite threshold amount, is found to prolong markedly the durations of tonic immobility in *Anolis*. The adrenalin is injected intraperitoneally (0.1 cc.), after diluting with Ringer's solution to concentrations varying with the experiments between 0.5:100,000 and 5.0:100,000. Controls injected with 0.1 cc. of Ringer's solution show no prolongation of immobility. The injections were made with the animal in the recording mechanism at 27.5°C. after one hour from the beginning of the experiment. The animal was not removed from the holder or otherwise disturbed at the time of injection. Figure 2 represents the number of normal spontaneous recoveries missed after adrenalin injection plotted as a function of the amount of adrenalin injected. The shaded circles are weighted averages of 80 experiments with many animals, the unshaded circles are unweighted averages, and the crosses are points for one individual animal. The curve is almost logarithmic, the slight deviation from the logarithmic probably having no theoretical significance (Hoagland, 1928). Probable errors are indicated on the plot by lines through the points perpendicular to the abscissa. The curve may be a measure of the rate at which the absorbed adrenalin becomes inactive, since the number of periods missed is measured in time units; or it may be a measure of the rate of diffusion of injected adrenalin from the body cavity into the blood stream. Such a diffusion effect would give a logarithmic function. The threshold previously suggested is found

in the case of injected adrenalin by extrapolation of the curve to the abscissa. It is approximately four parts per million of adrenalin in 0.1 cc. of Ringer's solution.

The logarithmic function is also what one would expect if adrenalin were decomposing according to a first order chemical process. The oxidation of adrenalin in the presence of excess oxygen would

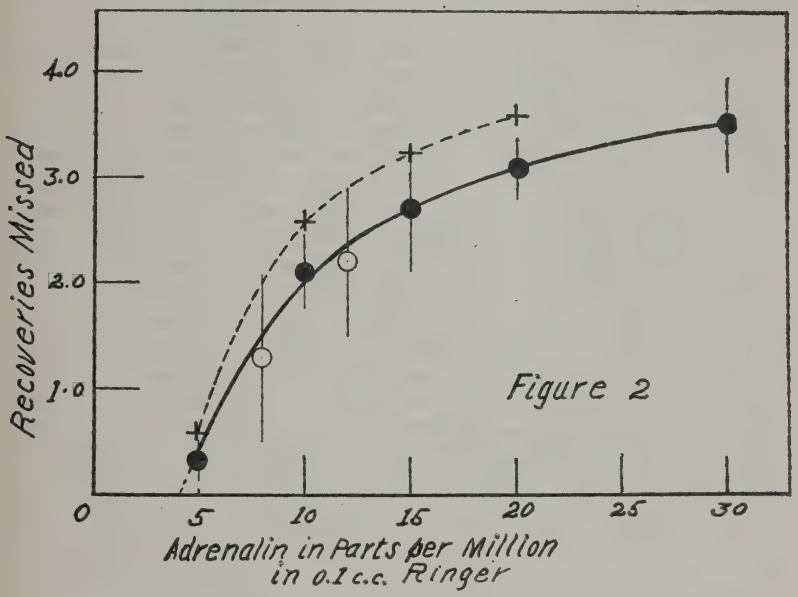

FIGURE 2

CURVE RELATING DOSE OF ADRENALIN TO NUMBER OF NORMAL SPONTANEOUS RECOVERIES MISSED

The shaded circles are weighted averages of eighty experiments with many animals, and the unshaded circles are unweighted averages. The crosses are points for one animal. The probable errors are indicated by lines through the average points perpendicular to the abscissa. A threshold for injected adrenalin is indicated by extrapolation. Controls injected with 0.1 c.c. of Ringer solution show no prolongation of periods of tonic immobility, i. e., no normal recoveries are missed.

give such a function. The temperature analysis of tonic immobility, it will be remembered, yielded two values of the critical thermal increment, μ, tentatively indicative of respiratory (oxidative) processes. It therefore seemed probable that if internally secreted adrenalin were one of the autacoids as assumed, which becomes inactivated through oxidation, that adrenalin first oxidized in the air and then

injected into the animal should have no effect on tonic immobility. This was found to be the case. Adrenalin allowed to stand in the air until oxidized, as indicated by the solution's turning pink, was found to have no effect whatever on tonic immobility. It is also noteworthy that the time required for minimum effective concentrations of adrenalin to oxidize in Ringer solution in the air is roughly comparable to the normal duration of tonic immobility at the same temperature.

While it is possible that adrenalin, on the basis of this evidence, may be one of the hypothetical autacoids which prolongs tonic immobility when present above a threshold concentration, this is not altogether certain. The injection of ergotamine, in the case of mammals, is known to paralyze the endings of the sympathetic nervous system. If it has a similar effect on lizards and if adrenalin acts peripherally in producing tonic immobility, one would expect that tonic immobility would be abolished by this drug. Exactly the opposite effect results. The duration of immobility is greatly prolonged by ergotamine. Since it is not certain how ergotamine affects the sympathetic nervous system of *Anolis*, this experiment is not in itself determinative. The fact that spinal reflexes may be elicited during tonic immobility is a strong argument that the effect is central rather than peripheral, since if the inhibiting autacoid acted peripherally one would expect it to block spinal reflexes. If, in this case, adrenalin acts *centrally*, any sympathetic paralysis due to ergotamine would not necessarily shorten or abolish tonic immobility. The prolonging effects of ergotamine may be due to general toxic effects which have nothing to do with the specific relations of adrenalin and the sympathetic nervous system to the mechanism controlling tonic immobility.[11]

While the effect of temperature on the durations of immobility, the continuance of the rhythm after the animal has been aroused and reimmobilized, and the adrenalin effects, all point to a hormone (possibly adrenalin) controlling tonic immobility, there are other facts to be considered that at first seem difficult to reconcile with this general picture. The onset of the condition is very sudden. It follows immediately with birds and mammals on merely turning them quickly dorsum downward. This is hard to explain in terms of a process *initiated* by a hormone. Moreover, it is noteworthy that the mere injection of adrenalin does not produce tonic immobility; it only prolongs the state if it is already existing.[12]

[11]A detailed description of the foregoing experiments and their implications will appear in the *Journal of General Physiology*, 11.

[12]In some experiments *Anolis* was injected with adrenalin but not immobilized until some minutes later. In these cases the duration of immobilization was also found to be increased.

The "shocking" effect on the centers of reflex tonus in the bulb and cord of suddenly removing an animal from the substratum and overturning it, or allowing it to struggle on its back with limbs in the air so that normal afferent stimuli from the feet and from the stretched muscles are abolished, must be considerable. At all times the muscles controlling body posture are in a state of tonus determined by nicely graded reflexes afferently controlled by the amount of stretch of the muscles and by the mechanisms of the internal ear (cf. Cobb, 1925, Magnus, 1924). The sudden overturning of an animal must result in a decided unbalancing, so to speak, of the central machinery of reflex tonus. In a very brief interval the tensions of most of the large muscles are changed which may be understood to result in a disorganized volley of afferent impulses sent to the internuncial neurones at the tonus centers. Since the animal's limbs are removed from the substratum there is no possibility of rapid readjustment by way of normal afferent stimuli and it seems not unlikely that such a condition might result in a promiscuous discharge of the tonus centers effecting an immediate "locking" of the entire musculature.

Such a violent discharge of a group of internuncial neurones might also be expected to influence the adrenal glands. Adrenalin might, therefore, be excreted in excessive amounts and, once in the circulation, act as we know that injected adrenalin does to prolong the state of tonic immobility. According to this hypothesis, the hormone in question does not in itself initiate tonic immobility but serves to maintain the predominance of those tonic impulses already passing and to prevent impulses from higher centers from gaining access to the final common paths to the muscles.[13] In the case of arthropods a similar mechanism may apply. Arthropods are often readily mobilized by merely lifting them from the substratum, thereby producing pronounced changes in the stretch of the limb muscles. While it is not likely that adrenalin is the autacoid involved in these cases there is good evidence, as Crozier has pointed out, to suppose that some inhibitory substance controls *the duration* of immobility.

To test this hypothesis it was decided to stimulate the presumed centers of reflex tone mechanically in an endeavor to produce tonic immobility. A fine needle was thrust into various regions of the nervous system of unanaesthetized *Anolis*. Stimulation of the medulla oblongata in most cases produced immediate tonic immobility, alike in all respects to that normally induced. This region, known to con-

[13]In the case of animals which are said to "feign death" when pursued by enemies the mechanism just outlined might conceivably be thrown into action by discharges from the thalamus or even higher centers, instead of by the disruption of afferent proprioceptive impulses as here suggested. This latter interpretation of the initiative of immobility, so far as it goes, is similar to that suggested by Verworn in 1898.

tain correlation centers of postural tonus, seemed to be the only one producing this effect when mechanically stimulated.

Since in these cases the needle was thrust into the floor of the 4th ventricle, one is performing essentially the equivalence of diabetic puncture, from which one might expect the release into the blood stream of adrenalin, producing hyperglycemia. This effect was clearly demonstrated by Claude Bernard in his classical experiments with rabbits and by other workers since. To test the effect of the puncture, and also in order to obtain an index of adrenalin under other circumstances, a series of blood sugar analyses was made by the titration micro-method of Hagedorn and Jensen (1926). The average results from analyses on 41 animals are summarized in the following table. From twelve experiments *in vitro* the analyses were found to be consistently about 25% too high in absolute units, but the method is *relatively* accurate to $\pm 2\%$, and it is the relative levels of blood concentration in which we are interested.

	Resting animals—Blood Sugar	Blood Sugar after 1½ mins. of tonic immobility	Blood Sugar after 1½ mins. of struggle	Sugar 1 min. after puncturing medulla	Controls 1 min. after punctures in other parts of nervous system	Average of blood sugar at intervals after injecting adrenalin 20 parts/million
mg/.1 cc.	.20±.01	.21±.01	.25±.02	.30±.01	.23±.01	.20±.01

From this table it is clear that there is quite an appreciable increase in blood sugar concentration accompanying puncture of the medulla, and since this has been shown in mammals to be due to release of adrenalin, it seems safe to assume that adrenalin release is responsible for the increase here. It will be noticed, however, that there is no appreciable increase in sugar after a minute and a half of tonic immobility, and also that the injection of adrenalin sufficient to make the animal miss several normal recoveries has no effect in increasing blood sugar. These facts are significant since they indicate that the mechanism of tonic immobility is more sensitive to adrenalin than is the mechanism of sugar release. From these data it further seems probable that adrenalin injected intraperitoneally is only slowly absorbed into the blood to a comparatively slight degree, and that the logarithmic curve of Figure 2 is actually a measure of the rate of diffusion of adrenalin into the blood stream, rather than the curve of a first order chemical process. This question can readily be settled by the application of the temperature method of analysis to the effect of adrenalin in prolonging tonic immobility.

The hypothesis that tonic immobility is produced by the discharge

of centers of reflex tonus through sudden changes in the tension of postural muscles, rather than by mechanisms of the internal ear was tested by destroying the ears of several *Anolis* (under ether anaesthesia). In these cases, after recovery, altogether normal rhythms of immobility were found to occur. Numerous attempts to remove or to denervate the adrenal bodies of *Anolis* in order to observe the effect on the duration of immobility, have so far proved unsuccessful owing to the smallness of the animals, the location of the adrenal bodies, and the diffuse nature of their nerve supply.[14]

Summary

A description is given of the phenomenon known as "death feigning," "animal hypnosis," or *tonic immobility,* and a method for examining its physiological mechanism is described. This involves in the main considering the quantitative relations between temperature and the durations of successively induced periods of immobility, in terms of possible theoretical interpretations and of the light which may thus be had upon specific chemical transformations within intact organisms.

The general significance of the method of temperature analysis for studies in animal behavior is discussed.

The probable physiology of tonic immobility in the lizard *Anolis carolinensis* is described in detail in the light of experimental evidence.

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[14] In addition to the question of the *mechanism* of tonic immobility, the condition itself offers opportunity for the study of tonus in skeletal muscle in the intact organism. The immobile animal resembles in many respects a decerebrate preparation. Certain aspects of deep reflexes are conspicuous, and experiments are in progress to measure the frequency of discharge of certain stretch reflexes of *Phrynosoma* as a function of the tension in the muscles.

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LE MÉCANISME DE L'IMMOBILITÉ TONIQUE ("L'HYPNOSE ANIMALE")

(Résumé)

On donne la description du phénomène de l'immobilité tonique, appelée fréquemment "l'hypnose animale," ou "faisant le mort." On discute cet état au point de vue de sa signification biologique et aussi comme moyen pour l'étude des processus nerveux centraux.

On présente ici une analyse du mécanisme physiologique de l'immobilité tonique chez le lésard *Anolis carolinensis*. Puisque ce travail exige, entre autres choses, la détermination de l'effet de la température sur la durée des périodes de l'immobilité comme indice de la dynamique, on donne un résumé général de la méthode d'analyse selon la température appliquée aux systèmes vivants.

Selon diverses expériences on suggère que l'immobilité tonique chez *Anolis* est produite par une décharge diffuse des neurones internonces de la medulla oblongata lesquels sont engagés dans la maintenance du tonus postural. On suggère que cette décharge dans les cas de l'immobilité causée par la manipulation est due à la désorganisation des impulsions afférentes proprioceptives par le stimulus immobilisant. Cette décharge en masse des centres médullaires, on suppose, cause la contraction des muscles du squelette et l'inhibition des centres plus élevés. On suggère que l'adrénaline libérée au même temps par une irradiation d'impulsions aux centres sympathiques tend à prolonger cet équilibre fonctionnel du système nerveux jusqu'à ce que la concentration de l'adrénaline tombe au-dessous d'une valeur de seuil.

HOAGLAND

ÜBER DEN MECHANISMUS DER TONISCHEN UNBEWEGLICHKEIT ("TIERHYPNOSE")

(Referat)

Man heschreibt die Erscheinung der tonischen Unbeweglichkeit, auch als "Tierhypnose," "sich tot stellen" und "playing 'possum" bekannt. Dieser Zustand wird in seiner biologischen Bedeutung beleuchtet, sowie als Mittel zur Untersuchung der Centralnervenfunktionen.

Eine experimentelle Analyse des physiologischen Mechanismus der tonischen Unbeweglichkeit in der Eidechse, Anolis carolinensis, wird dargelegt. Da diese Arbeit—unter andern Vorgängen—auch die Bestimmung der Wirkung der Temperatur auf die Zeitdauer der Unbeweglichkeit, als Nachweis der zugrunde liegenden Dynamik, mit einschliesst, so wird ein allgemeines Resumé der Temperaturmethode der Analyse, und ihre Anwendung auf den lebenden Organismus, mitgeteilt. Verschiedene, experimentelle Ergebnisse deuten darauf hin, dass die tonische Unbeweglichkeit in Anolis, durch eine starke Entladung internunzialer Neuronen in die Medulla oblongata hervorgerufen wird, welche zur Erhaltung des Stellungstonus beitragen. Man vermutet das diese Entladung—in Fällen wo die Unbeweglichkeit durch Manipulation erzeugt wird—durch die Auflösung der propriozeptiven Impulse hervogebracht wird, ausgelöst durch den Reiz der die Unbeweglichkeit verursacht. Diese Massenentladung der medularen Zentern verursacht vermutlich, Kontraktion der Hauptmuskeln und Hemmung der höhern Nervenzentern. Man deutet an, dass Adrenalin, gleichzeitig ausgelöst durch die Ausstrahlung der Impulse zu sympathischen Nervenzentern, eine Verlängerung dieses "Gleichgewichts" der Funktionen des Nerven-systems bewirkt, bis das die Adrenalinkonzentration unter den Schwellenwert sinkt.

HOAGLAND

МЕХАНИЗМ ТОНИЧЕСКОЙ НЕПОДВИЖНОСТИ (ГИПНОЗ ЖИВОТНЫХ).

(Реферат).

В этой статье описывается явление тонической неподвижности, часто называемой «животным гипнозом», «мнимой смертью», «притворяться как оппосум. Это состояние рассматривается с точки зрения своей биологической важности, а также, как способ исследования процессов центральной нервной системы.

Изложен экспериментальный анализ физиологического механизма тонической неподвижности ящерицы Anolis Carolinensis. Так как, среди других процедур, эта работа заключает в себе определение влияния температуры на продолжительность периодов окоченения, как показатель подлежащей динамики, то здесь дается общее резюмэ температурного метода анализа, как он применяется к живым организмам. На основании различных экспериментальных подходов предположено, что тоническая неподвижность Anolis порождается распространяющимся раздражением интернунциальных нервов продолговатого мозга, участвующих в поддержании тонуса позы.

Далее предположено, что разряжение в этих случаях окоченение посредством манипуляции возникает благодаря дезорганизации афферентных проприоцептивных импульсов соответственными стимулами Это массовое разряжение центров продолговатого мозга вызывает сокращение скелетных мускулов и торможение высших центров. Предполагается также, что адреналин, освобожденный в это время иррадиацией импульсов в симпатические центры, способствует продолжению этого функционального «баланса» нервной системы, покуда концентрация адреналина не падает ниже порога.

ГОАГЛЕНД
(Hoagland)

A MATHEMATICAL ANALYSIS OF WORK CURVES*

From the Psychological Laboratories of Columbia University

M. R. Neifeld and A. T. Poffenberger

The material presented in this paper is a portion of an extensive investigation of the effects of continuous functioning upon seven forms of physical work and four forms of mental work in which eleven subjects took part over a period covering a number of months.[1] It represents an analysis of a sampling of the work curves for different individuals and for the same individual when engaged in different kinds of work in order to determine their form and to translate them into mathematical equations.[2]

An attempt was made by Ioteyko[3] to express her work curves in the form of equations by the process of curve fitting. She found that her curves best fitted a parabola of the third degree whose equation is

$$y = a + bx + cx^2 + dx^3$$

She stresses the point that the signs of the constants a and c are always positive, and that the signs of the constants b and d are always negative for all her subjects. She assumes—on just what grounds it is not clear—that the constant a is given by the maximum initial contraction. There does not seem to be any apparent justification for this assumption, particularly when it is considered that the value so chosen is a purely accidental one. It would seem as if the more rigorous procedure would be to calculate the constant a in the same manner as the other constants b, c, and d. Each one of these, b, c, and d, is taken to represent one of three component forces which make up the fatigue curve. The constant b is identified with the consumption of stored-up carbohydrates in the stimulated muscle. Since b is always negative and is associated with the first power of x, the consumption of stored-up carbohydrates is directly proportional to the time elapsed. As the muscle keeps on working, equal amounts of energy are consumed in the successive intervals of time. The tendency of this factor is, therefore, to draw the work curve sharply down.

The constant c is identified with the release of nervous energy. It tends to make the curve rise. As the amount of resistance

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[1]The investigation was made possible by the support of the Ernest Kempton Adams Research Fellowship.

[2]The first-named author is responsible for the mathematical work presented in this paper.

[3]Ioteyko, J. La fatigue. Paris, 1920.

against which the muscle is working increases, there is a release of greater amounts of nerve energy. This increase is associated with the square of the time. In each equal interval of time there is liberated a greater and greater amount of energy. But as the numerical value of the *c* constant is smaller than that of the *b* constant, its effect on the character of the curve is not so great.

Finally, the negative *d* factor tends to draw the curve downward, but since its numerical value is the smallest of all the constants, its effect is the least appreciable. In its own order of magnitude, however, its effects increase very rapidly because it is associated with the cube of the time. Physiologically, it is interpreted as the effect of the formation of toxic products. At the beginning of stimulation there is practically none present, hence the small numerical value of *d*. At no time does it become relatively very large, but it increases as the cube of the elapsed time.

The work curves to be studied in this report were obtained in every case by requiring the subject to continue work until he was exhausted. The curves, composed of series of muscular contractions, were of varying lengths for different subjects and for different forms of work. The curve of one subject might consist of 47 contractions, another 53 contractions, and another 58 contractions. In order to obtain data suitable for comparison, the varying numbers of measures in the curves were arbitrarily divided into 15 sections or units. Each work curve as treated hereafter will therefore consist of 15 sections. This is not the most rigorous method that might have been used, but it was used for practical purposes. A series of work curves from this same individual working under identical conditions may readily be combined by averaging scores for various sections of the curves. Thus, in the case of lifting a 24-pound

TABLE I

AVERAGE OF NINE WORK CURVES FOR ONE SUBJECT—ARM CONTRACTION

Section No.	Units of Work
1	25.1
2	25.2
3	24.8
4	24.3
5	23.8
6	23.3
7	22.2
8	20.9
9	18.5
10	15.8
11	12.1
12	9.7
13	5.7
14	3.4
15	2.1

weight by bending the arm at the elbow, one individual produced 9 separate curves of varying length. With the method given above we obtain the data presented in Table I.

These measures are plotted in Figure I (solid line), in which the ordinates show the height of contraction and the abscissas show the

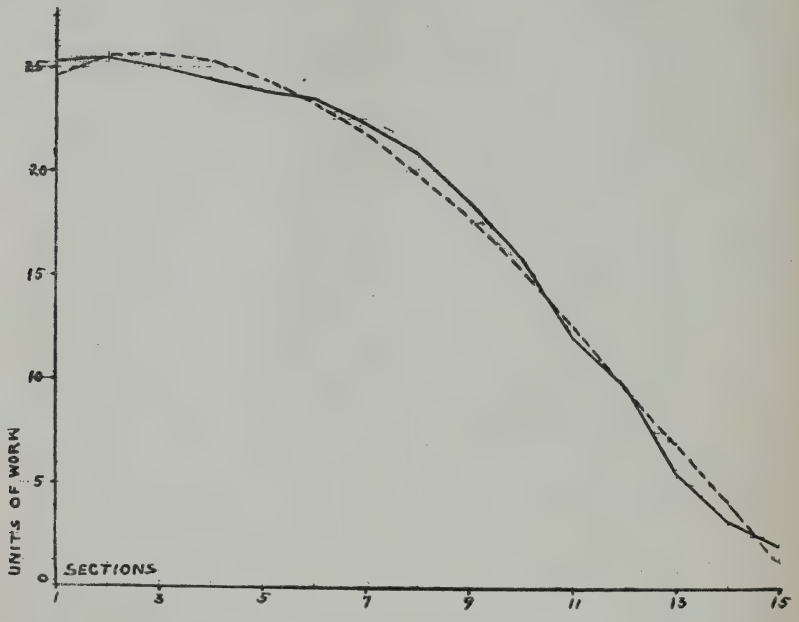


FIGURE I

ORIGINAL AND CALCULATED CURVES FOR TYPICAL PERFORMANCE—ARM CONTRACTION

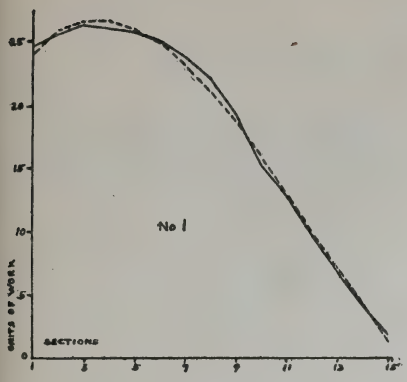
(Solid line is original and broken is calculated)

successive intervals of time or the sections of the curve. The form of the graph suggests that we are dealing with a parabolic trend.[4] A preliminary calculation shows that a parabola of the third degree would give a very close fit to the observed values. In fact, the third degree parabola gives a remarkably close fit in the case of nearly all the curves studied, just as in the work reported by Ioteyko. In this general equation

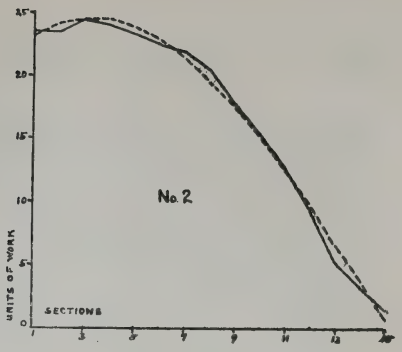
$$y = a + bx + cx^2 + dx^3$$

the height at any point of the curve above the base line is a function of the time elapsed (x). a, b, c, and d are the constants to

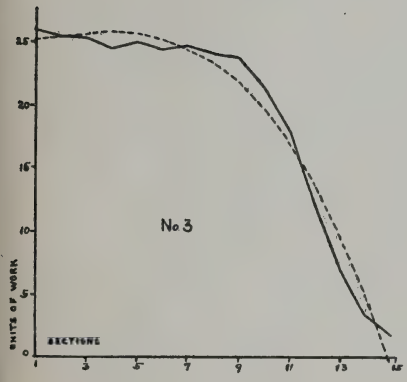
[4]Rietz, H. L., *et al.* Handbook of mathematical statistics. Boston: Houghton Mifflin, 1924.



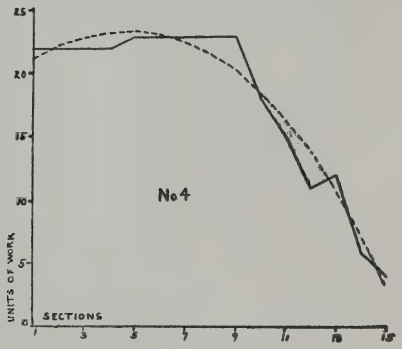
1. Good



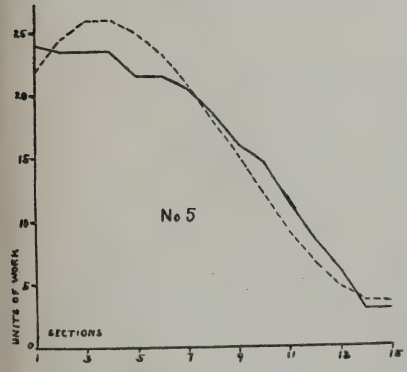
2. Good



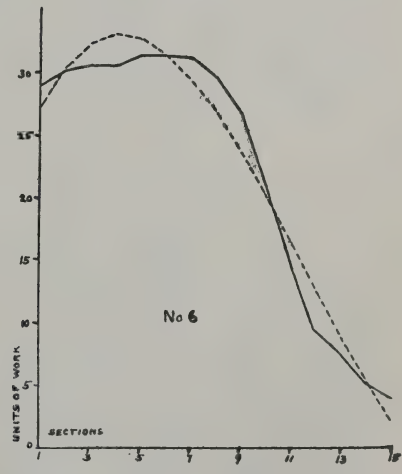
3. Fair



4. Fair



5. Poor



6. Poor

FIGURE II

SAMPLES OF GOOD AND POOR FIT

(1 and 2 are "Good"; 3 and 4 are "Fair"; 5 and 6 are "Poor")

perature sensitivity. The fact of the constricting of the vessels with the cooling and the dilating with the warming suggests that it quite likely functions as a stimulus-mediating agent. At cold spots, contraction may be the adequate stimulus for the nerve, while at warm spots dilation is the stimulus. Thus the temperature nerve would be a kind of centripetal blood-vessel nerve. Here Goldscheider thinks that a definite calibre of the blood-vessel would be necessary for a particular temperature, perhaps as in the case of mercury in the thermometer. We have been accustomed to think of the change in the content of the vessels as initiated by the preceding nerve processes, however, and it would seem that the fact that the nerve-fibers project beyond the vessels and into the epithelium speaks decidedly against the centripetal character of blood-vessel nerves. In other words, if the blood-vessels are the end-organs, why do the nerve-fibers extend beyond the vessels and into the epithelium? Goldscheider believes rather that the vessels are placed in close proximity to the terminal temperature apparatus in order that the former may serve to bring the temperature of the latter back to normal quickly after hot or cold stimulation. The vessels furnish, he thinks, an efficacious compensation for injurious temperatures.

As mentioned above, Goldscheider did not find, as he had hoped to do, an anatomical differentiation between the two kinds of temperature receptors, warm and cold. He states, however, that menthol acts chiefly on the nerves for cold sensitivity, and that it is much less irritating for those for warmth sensitivity. Herz is quoted as stating that in the case of pressure exerted upon nerve-trunks, as when the limbs "go to sleep," the cold sensitivity disappears first and, much later, the warmth sensitivity.

It is a noteworthy fact that Goldscheider found no tactile corpuscles beneath the pressure spots which he had marked and extirpated. He believes it to be true beyond a doubt that the so-called tactile corpuscles stand in direct relationship with the sense of touch, for they are most numerous in the especially sensitive tactile areas of the body, i.e., the fingertips, palms, and the soles of the feet. The question is, however, relative to the nature of this relationship. The hand is sensitive to the finest and most delicate of mechanical stimuli; yet it is at the same time exposed to the most violent of shocks and the heaviest of pressures. Think of the pressure to which the subepithelial nerve terminations in the hands of laborers and gymnasts are exposed; yet they remain sensitive. The corneal layer

is much thicker in the palms of the hands and the soles of the feet than in any other region of the body, thus furnishing the necessary protection for these parts. May it not be that the corpuscles do not function as end-organs, but rather serve as an additional protection for the nerve-endings in these regions? Goldscheider cites the equal distribution of corpuscles in the hands and feet of apes as indicating that they do not bear a relationship with the tactile sensitivity which we have acquired in the hands, but rather with the use of the extremities for work. Furthermore, the number of corpuscles does not correspond to tactile sensitivity in every instance; for example, fewer corpuscles are found in the tip of the tongue than in the fingertip, although tactile sensitivity is greater in the tongue than in the fingertip. We might add to Goldscheider's evidence another point. Histologists describe many corpuscles, some of a quite specialized type, which are to be found in the bill of the duck, in the bills of other birds, and in the snout of the pig. The bills and snouts of animals are members which, so far as we know, do not possess extremely delicate tactile sensitivity, but they are subjected to a great deal of wear and tear and pressure of the kind which we might reasonably suppose might call for protective sheaths for the nerve-terminations. Why, then, we may ask, are there in the hands and feet some sensory nerve-terminations which have no protective capsules? The answer may be that these endings belong to modalities which have higher thresholds of sensitivity, and which need not be so highly developed and delicate as are the finely discriminating tactile terminations. Thus they do not need the protection of a capsule. It is a known fact that the threshold of pain sensitivity is higher than that for the other modalities. It may very well be that the more elaborate terminal apparatus of nerve-fibers, which is characteristic of those which are encased in capsules, is demanded as a compensation for the greater resistance offered by the protective sheaths.

Goldscheider's extirpation experiment was repeated some years ago by Dallenbach, with the result that no corpuscular nerve-terminations were found beneath the sensitive points marked.

III. The Present Problem

It would seem that, with a few exceptions, the assumption of functional receptor specificity is based largely upon speculation and upon experimental data derived from experimentation upon limited areas of the human body, viz., the cornea and conjunctiva of the

eye, the glans penis, and the mouth. A considerable amount of evidence has been gathered in support of the view that in these regions the Krause end bulb functions as the specific cold receptor. Especially in the eye does this seem to be the case. This has led to the generalization that the Krause bulb is the end-organ for cold sensitivity, implying that it functions as such throughout the body, or in all regions where cold spots exist. The fact that the Ruffini corpuscle lies at a deeper level in the skin, coupled with the fact that the threshold for warmth sensitivity is considerably higher than that for cold sensitivity, has led to the conjecture that this particular corpuscle functions as the specific receptor for warm stimuli. There is more evidence for the existence of a definite relationship between tactile sensitivity and Meissner corpuscles and hair follicle innervations, although, as Goldscheider has pointed out, there are reasons for believing that the anatomical features which cause the former to be placed in the corpuscle category are not integral parts of the receptor apparatus, but that they function merely as supporting and protective structures. According to this view, corpuscles as such are not receptors.

The aim of the present work is to examine microscopically samples of skin taken from several regions of the human body in order to study the distribution of terminal corpuscles in general, and to see if certain of them exist in numbers sufficient to account for the modalities of sensitivity with which they have been associated.

A. *Material.* Samples of human dermal tissues from seven bodily regions were examined, these regions being determined largely by what tissue medical examiners and surgeons were able to obtain for us. They were as follows: thigh along the sartorius muscle, abdominal epigastric region, left breast at the nipple, left breast below the nipple, middle of the sole of the foot, chest, leg along the tibia. As much subcutaneous tissue as possible was taken along with the dermis, and care was taken to select normal tissue in every case. The specimens were all in good condition when obtained for sectioning, having been kept but a short time, and in a refrigerator, and in many cases it was possible to place the tissue in fixing fluid immediatley upon the removal from the donor. A fairly large piece of tissue was taken in most cases, being sometimes as large as two and one half by five or six centimeters. These were placed immediately in a 10% solution of formalin, which, in addition to preserving the tissue, acts as a very satisfactory killing and fixing

fluid. In some cases Zenker's fluid was used, but with less success for our purposes.

B. *Method.* Tissue preserved in formalin was cut into pieces from two to five millimeters in width and five millimeters to one centimeter in length for sectioning. These small pieces were allowed to remain in 10% formalin for several hours to insure thorough fixation, which might have been incomplete in the undivided pieces of tissue. As we found it impracticable to follow any prescribed technique closely, our process of hardening will be described.

From the formalin the tissue was placed without washing in 15% alcohol, and then up through a graded series of alcohols; 25%, 35%, 50%, 85%, 95%, and absolute, remaining in each alcohol solution for a period of from 12 to 24 hours, a much longer time than is usually prescribed by handbooks of histological technique. This process dehydrates and at the same time hardens the tissue. From the absolute alcohol the tissue was placed first in a mixture of xylol and absolute alcohol in the proportion of one part of the former to three of the latter, then in a half and half mixture of the same, next in a mixture of one part absolute alcohol and three of xylol, and finally in clear xylol, remaining in each of the mixtures from 12 to 24 hours. When cleared, the tissue was placed in a solution of paraffin in xylol for infiltration. Infiltration in melted paraffin in the paraffin oven followed, the tissue usually remaining in the oven about 12 hours. In order to obtain good sections it was found necessary to lengthen the period of fixing in the graded alcohols and the period of infiltration considerably over the time specified by most manuals of histological technique.

The tissue was imbedded in paraffin and serial sections were made with a rotary microtome. Staining was by the so-called "ordinary" stains. Delafield's haematoxylin was used as a nuclear stain and eosin as a counter stain. In some cases acid fuchsin was employed as the counter stain, but on the whole eosin was found to produce more satisfactory results.

The sections were cut 10 and 12 micra in thickness, and at right angles to the long axis of the rectangular pieces, the microtome knife being made to strike the subcutaneous tissue first and the corneal layer last. In this way folding of the corneum over the deeper layers was prevented. When arranged in serial order on the slides, the sections assumed a uniform position in all the series; the surface of the skin (corneum) was always at the right, appear-

ing at the left when viewed under the microscope. The number of sections per slide averaged about 35, so that a set of 12 or 15 slides represented a strip of tissue five and a half or six millimeters in length. The blocks of tissue varied between three and five millimeters in width, so that the area covered by the average set of 12 or 15 slides varied between 18 and 30 square millimeters. A few of the series were much shorter, however.

The use of ordinary stains was not decided upon until many trials had been made. Our original plan was to make use of special nerve stains, so that the nervous tissue of the corpuscles might be differentiated, but after trying several it was decided that such stains are too capricious to enable us to obtain quantitative data. It is extremely difficult to obtain a complete series of sections of tissue stained or impregnated with nerve stains. We found, however, that corpuscular nerve terminations can de detected easily through the use of haematoxylin and eosin, even though these stains do not enable these structures to be studied in detail. The nervous tissue elements are not distinctly differentiated, but the connective tissue of the capsules is rendered quite distinct, as demonstrated not only by our own preparations, but also by those of Dr. Thyng and Dr. Sykes of Tuft's Medical School, in which Pacinian corpuscles were easily identified in the subcutaneous tissue of the index finger.

In order to test our method for its efficiency in differentiating the connective tissue of the corpuscles, the tissue taken from the sole of the foot was searched first, as there are known to be Meissner corpuscles in this region. Three series were examined, comprising a total of about 900 sections, representing a skin area of about 32 square millimeters. The middle of the sole of the foot is known to contain but few Meissner corpuscles as compared with the finger tips, palms, or even the toes, but within the area examined we were able to observe two bodies which were unmistakably Meissner corpuscles, well differentiated from the surrounding tissue. All parts of the corpuscles were plainly visible with the exception of the coils of the nerve-fiber. Their dimensions were in the one case 80 micra long by 40 wide, while in the other they were 96 by 48 micra. Ruffini (19), in his summary of work on sensory nerve-terminations, gives the dimensions as varying between 30 and 108 micra in length and 30 and 100 micra in width, so that our measurements are seen to correspond and may be taken as one check upon our method.

Having satisfied ourselves that corpuscles were being differentiated sufficiently by our stains, we proceeded to search through our whole stock of sectioned tissue for any terminal corpuscles. Having observed Meissner and Pacinian corpuscles in good haematoxylin-eosin sections, we see no reason why, if the sheaths of these corpuscles can be differentiated so easily, we should not also be able to detect the presence of other forms which differ principally in size and shape.

The inspection of the sections was made as systematically as was possible. They were taken in their serial order and scrutinized with both low and high power objectives. Bausch and Lomb 16mm. and 4mm. objectives were used with 5X and 10X oculars of the same make, affording magnifications of 50, 100, 215, and 400 diameters. The following series were studied, including the foot tissue already mentioned.

	Series	Slides	Sections	Approximate Area
1) Sole of foot	1	14	530	19.08 sq. mm.
	2	5	206	7.41
	3	4	157	5.65
2) Thigh	1	15	640	23.04
	2	15	530	19.08
	3	9	384	13.82
	4	22	375	13.50
3) Leg	1	18	620	22.32
4) Epigastrium	1	8	240	8.64
	2	14	430	15.48
5) Chest	1	11	430	15.48
6) Breast below nipple	1	11	425	15.30
7) Breast at nipple	1	10	310	11.16
			5277	189.96 sq. mm.

In all, 5277 sections were examined, representing approximately 190 square millimeters of skin surface.

C. *Observations.* In the hairless tissue of the sole of the foot Meissner corpuscles were observed with great facility, but no other types of corpuscles were found there.

In none of the 4384 sections of skin taken from hairy regions of the body were terminal corpuscles of any kind encountered. Our sections did not always include the whole depth of the subcutaneous tissue, making it impossible to state whether or not there were Pacinian corpuscles in these regions. However, in all cases we were able to examine thoroughly the whole of the chorium and the epider-

mis. Especial attention was given the papillary layer and the tissue immediately below it, where the Krause bulbs, the alleged end-organs for cold sensitivity, are supposed to lie. However, no Krause bulbs were found.

D. *The Significance of the Facts Observed.* The apparent absence of differentiated nerve-terminations in the form of corpuscles throughout large areas of the hairy regions of the human body makes certain theories of specificity appear doubtful. We have admitted that the Meissner corpuscles and the hair follicles may be the end-organs for superficial light pressure or tactile sensitivity. But the dearth of corpuscles of any kind in some of the hairy regions makes it impossible for us to believe that the Krause bulb or any other corpuscular termination is the specific cold receptor, or that the Ruffini corpuscle is the specific warmth receptor. Our findings are in agreement with those of Pendleton, who hunted for end bulbs in the hairy regions of the skin, using non-haematoxylin stains, and with those of Dallenbach, who repeated some of Goldscheider's work, as described above. Reports of the work of Pendleton and Dallenbach are as yet unpublished.[2]

Pendleton, in a private communication, quotes Ruffini as claiming that no Krause bulbs have ever been found in the hairless parts of the hands and feet, though he does not give the specific reference. He states that von Frey assumes that the Golgi-Mazzoni bulbs are forms of the Vater-Pacini corpuscles, which have been found in the hairless portions of the hand and foot, in tendons, mesentery, and other places, but we find no mention of anyone finding them in the hairy parts of the human body, with one exception—von Frey found one in the arm, as noted in the previous discussion of his work. As Pendleton points out, if they were present they should be easily visible, because they are 50 micra long, they are surrounded by a distinct sheath, and they can be seen in many sections of the hairless parts of the body.

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LA SENSITIVITÉ DU DERME ET LES BOUTS DIFFERENCIÉS DES NERFS DE LA PEAU

(Résumé)

L'auteur passe en revue l'évidence positive et négative pour les corpuscules des nerfs comme les organes de bout de la sensitivité du derme et ensuite présente son propre travail qu'on est obligé de considérer comme évidence négative.

Cette étude est un rapport de l'examen histologique de 5277 sections représentant 190 mm. carrés de la surface de la peau de treize parties du corps. On a employé l'hématoxyline de Delafield comme teinte nucléaire et l'éosine (quelquefois acide fuchsine) comme teinte de contrôle.

Le tissu de la plante du pied a contenu des corpuscules Meissner mais dans les tissus des parties couvertes de poil on n'a pas trouvé de bouts spéciaux. Une revue des données d'autres observateurs ne montre aucune différence à l'exception que von Frey a trouvé *un* corpuscule Dogiel dans la peau du bras.

On prêtera toute l'étude, qui contient une discussion beaucoup plus détaillée que celle de ce résumé, si l'on s'adresse aux Laboratoires Psychologiques, Clark University.

GILBERT

ÜBER DIE EMPFINDLICHKEIT DER HAUT UND DIE DIFFERENZIIERTEN NERVENENDIGUNGEN DER HAUT

(Referat)

Der Vefasser führt die Beweise für und gegen die Theorie an, dass die Nervenkörperchen die Endorgane der Hautempfindlichkeit bilden, und berichtet dann seine eigne Arbeit, die man als negativen Beweis hierfür betrachten muss.

Die vorliegende Abhandlung berichtet die histologische Untersuchung von 5277 Schnitten, die ungefähr 190 quadrat mm. der Hautoberfläche verschiedener Körperteile entnommen waren. Man benutzte Hämatoxylinlösung (Delafields') als Kernfarbe und Eosin (mitunter saures Fuchsin) als Gegenfarbe.

Das Bindegewebe der Fuszsolen enthielt Meissnersche Körperchen, doch in dem Bindegewebe behaarter Teile fanden sich keine besondern Endigungen irgendwelcher Art. Die Befunde anderer Forscher stimmen hiermit überein, mit der einzigen Ausnahme dass von Frey *ein* Dogielsches Körperchen in der Haut des Armes fand. Die Original Abhandlung, die eine viel Gründlichere Erörterung des Problems enthält, wird auf Anfrage im psychologischen Laboratorium der Clark Universität gern geliehen werden.

GILBERT

КОЖНАЯ ЧУВСТВИТЕЛЬНОСТЬ И ДИФЕРЕНЦИРОВАННЫЕ НЕРВНЫЕ ОКОНЧАНИЯ КОЖИ.

(Реферат).

Автор дает обзор положительных и отрицательных данных о нервных тельцах, как концевых органах кожной чувствительности, и затем излагает свой собственный труд, который должен подкрепить отрицательные данные по этой проблеме.

Работа представляет собой сообщение о 5277 гистологических срезах, охватывающих 190 кв. мм. кожной поверхности с тринадцати различных мест тела. Для окраски ядер был взят гематоксилин Delafield'a, а в качестве противоположной ему краски—эозин (в некоторых случаях—кислый фуксин).

Ткань, взятая с подошвы ноги, содержит мейснеровы тельца, но в тканях, взятых с мест, покрытых волосами, не были найдены специализированные окончания подобного рода. Обзор ряда других работ не обнаруживает расхождений с указанными данными, за исключением работы Фрея (Frey), нашедшего единственное тельце Догеля в коже руки.

Оригинальная работа, содержащая значительно больше подробностей, чем настоящая статья, будет опубликована в трудах психологических лабораторий Кларкского Университета.

Джильберт (Gilbert).

THE FUNCTIONAL RÔLE OF FINGER TOUCH AND DAMPER-PEDALLING IN THE APPRECIATION OF PIANOFORTE MUSIC*[1]

From the Psychological Laboratories of The Johns Hopkins University

CHRISTIAN PAUL HEINLEIN

In the appreciation of pianoforte music, critical judgments are often passed by the listeners concerning the so-called "finger touch" of the performer. If you should make an inquiry into the opinions held by any large audience of appreciative listeners familiar in a general way with piano music and technique, you would probably be surprised to learn that the majority entertain the singular notion that a performer's interpretation of a given composition is related in some strange manner to a mystical spirituality inherent in keyboard touch. You will hear it said that a certain rendition is beautiful because the performer possesses "a marvellous, mysterious touch." In some few cases, the phrase "piano touch" is employed interchangeably for facility in keyboard technique; however, such judgments are often conveyed with the intention of ascribing to keyboard touch something quite mystical, something spiritual, something inexplicable and unanalyzable, something, as it were, transcending the fundamental characteristics common to all sentienda. Auditors, emotionally enthralled by the vague, transient charm of music yet totally ignorant of the few physical principles which underlie tonal expression, have been heard to exclaim that a personal, magnetic force is often conveyed from the fingers of the artist to the keys of the piano.

Strike a piano key, and what do you have? A resultant tone with position in space and time, subject to change in extensity (1, 7), intensity, protensity, and quality. Only this and nothing more. Piano touch per se is not a separate attribute of the tonal complex, and it does not in itself impart one single iota more to the entire scheme. There is no sensorial condition which transcends dependence upon these fundamental characteristics common to

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[1]The investigation recorded in this paper was carried on during the tenure of a National Research Fellowship in the Biological Sciences.

all sentienda. This is a harsh dictum, indeed, to many of those music-lovers who orthodoxically adhere to the belief that artistic pianoforte performance is bathed in many irreducible and unfathomable mysteries. This dictum is unpopular and unacceptable to the music-lover because it is bounded by some simple laws of mechanics, and any reference to mechanics is taboo in an art which is held to be essentially human (4).

Unfortunately, so many listeners direct their attention to finger, hand, wrist, and arm movements, as if in these movements lay the whole secret of individual interpretation. No one will deny for a single moment that many types of bodily movement function in the formation and development of the performer's various emotional attitudes, and that these same movements may to a large degree impress, react upon, and bias the general receptivity of the auditors. Considered from this angle, it is of no wonder that one artist will produce a different effect upon an audience than will another, or that a baton-wielder of great flexibility will enthuse in an entirely different manner than another conductor of less muscular motility (8, chap. 2 and 3). However, one must remember that, although possessing an important value in themselves, these conditions are extrinsic to the tonal complex. In the case of piano performance, if these extrinsic conditions are eliminated, a tone related in a specific manner to the characteristics limiting it is heard the same whether the sound is produced by one type or by another type of finger touch, wrist and arm movement, or by the mere mechanical stroke initiated by a wind bellows (6).

Facility in finger technique, the acquisition of accurate keyboard control, the ability to coordinate complex moving finger patterns, agility in the production of intensity shadings through appropriate changes in amplitude of stroke, motor synchronization with devious rhythmic patterns anticipatorially perceived—these and many other like features condition any performer's interpretation. The importance of these several methods of control leading to various types of interpretation must not be minimized. Nevertheless, it is quite possible to keep these and many other keyboard conditions, which I have not taken time to enumerate, constant throughout, and yet to present a number of different interpretations, effected by a means usually overlooked, that have to the listener little or nothing in common. In two successive renditions of a given composition, a performer may finger the melodic and harmonic progressions involved

in precisely the same manner, he may in each instance strike definite keys with the same intensity (assuming for the sake of argument a resultant identity of tonal amplitude), he may prolong in each case specific series for an exact temporal extensity, and despite such constancy of conditions he may supply his listener with two very different interpretations. The difference thus effected is not the result of any modification of direct keyboard control (despite the fact that most auditors would unreflectingly pronounce the change a finger-touch modification), but the result of a use of entirely *different damper-pedal patterns.*

The fact that damper-pedal differences are commonly perceived as differences in finger touch is quite convincingly demonstrated in the results obtained from a very simple experiment involving the judgments of 16 selected male college students, eight musically trained and eight musically untrained. These students, all members of a class in general psychology at The Johns Hopkins University, attended the test periods both individually and in small groups of three or four. Provided with comfortable chairs, the test subjects were seated facing a concert-grand model of the Duo-Art reproducing piano,[2] which was located at a hearing distance of approximately 20 feet.

Before experimentation was begun, the subjects were required to read carefully the following sheet of instructions:

> You will hear two renditions of a single musical composition played for you on the concert-grand Duo-Art reproducing piano. You are requested to listen attentively to each performance for the purpose of rendering comparative judgments.
>
> In comparing the two renditions, you are asked to ascertain whether the presentations are identical or different. If you decide that the presentations are different, attempt to ascertain the nature of the differences which you have noted. At the conclusion of the second composition, you will be asked to make a brief note of your judgment in writing.

The music chosen for presentation was Victor Herbert's composition entitled "Kiss Me Again," recorded by Robert Armbruster and played on the Duo-Art reproducing piano (see and hear Duo-Art reproducing roll, no. 67168, Meditations, no. 1). This com-

[2]The writer wishes to express his deep appreciation and thanks to the Aeolian Company of New York City for their kind cooperation and generous loan of the Duo-Art reproducing piano which made possible this experimentation.

position was chosen for its strong melodic features, its relative simplicity of form, its wide familiarity, and its universality of appeal among the student body. A less familiar piece of any greater form complexity would demand on the part of a listener more concentrated attention to secondary features, make recall more difficult, and in turn vitiate against any simple comparison in judgment (3).

In the first rendition, the composition was played at normal tempo and intensity according to roll indications. In the second rendition, which immediately followed the first rendition after re-roll, the tempo and intensity characteristics were maintained constant with the first rendition, while tonal duration was altered through complete elimination of the damper-pedal action. Tonal duration in the second rendition, therefore, was dependent upon the actual length of note perforation on the roll. The effect produced by this simple change in technique is very striking indeed. Minus pedal action, the melodic contour is fairly well sustained, while the root of the bass which supports the accompanying harmonic figures is frequently played staccato. Elimination of the damper-pedal causes the well-rounded arpeggios to sound sharp and brittle (7, p. 250), while the harmonic background suffers a flippant touch of the jazz in its bare rhythmic beat. Absence of the damper-pedal effects means absence of tonal flux. Pitch points become sharply outlined and often offend through their transiency and evasiveness. The composition is no longer bathed in the delightful richness of soft mellow timbres produced by subtly delicate overtones. The presentation assumes a harshness of unconventionality which neither pleases nor offends directly. Nevertheless, a single simple comparison is sufficient to bear upon and impress the most naïve listener that without damper-pedal effects something essential to artistic interpretation and expression is sorely lacking.

At the conclusion of the second rendition, each subject was given a second sheet on which were printed the following questions:

1) Were the two successive renditions of the composition played exactly the same? Answer yes or no.

2) If you think that the two renditions differed from each other, state the nature of the differences noted.

After the subject had completed his answers to these questions, he was given a third sheet which presented the following form:

The following list includes musical factors which might have been varied in the two renditions which you heard. Place a check-mark beside those

factors which you consider were varied. If there were other factors varied than those listed below, indicate in writing what these were.

The variations which I noted were variations in:

1) Intensity of tone
2) Pitch
3) Touch
4) Time
5) Phrasing
6) Tonal duration
7) Rhythm
8) Accent
9) Dynamics
10) Timbre

Of the above list of factors which you have checked or further noted, which particular factor do you consider was varied the most frequently?

The subject was not allowed to turn this sheet until he had completed his answers. Upon completion, he was requested to answer the questions which were listed on the reverse side. These final questions follow below:

1) Was pedalling employed in either direction?
2) If so—
   What was the nature of the pedalling?
   Was the pedalling the same in both compositions?
3) If you have decided that pedaling was employed, and that the pedalling for the two renditions was not the same, in which rendition was pedalling used the most frequently?
4) What type of pedalling was this?

Of the 16 subjects tested, each subject recognized marked differences between the two renditions. Every subject was familiar with the composition, and preferred the first rendition to the second. A few characteristic replies to the second question of the second sheet follow:

"The first rendition was played with a heavy legato touch which gave the piece an expression of deep feeling. The second rendition was played with a distinctly staccato touch which tended to destroy feeling."

"They differed in the touch of playing."

"The first rendition was easy flowing and pleasing as well as soothing to the ear, while the second was in a short jerky movement not soothing to the ear."

Fifteen of the 16 subjects declared that variation in finger touch produced the chief differences in interpretation. Other factors which

were considered varied in order of frequency were: tonal duration, 14; time, 10; rhythm, 10; phrasing, 9; accent, 9; intensity of tone, 8. No subject mentioned pedalling until this factor was drawn to his attention in question form on the final information sheet. It is also interesting to note that the fact of pedal change produced a number of illusions in judgment. The rendition without pedal was judged to be more rapid in tempo, more accentuated with greater intensity of tone, and more akin to fox-trot rhythm than to waltz rhythm (4, p. 86). One subject remarked that the second rendition was "jazzy." This leads one to believe that tonal duration of an appreciable length tends to vitiate against the musical complex recognized as "jazz."

In recapitulating, it is quite apparent from this brief study that both musically trained and untrained subjects tend to perceive damper-pedal differences primarily as differences in finger touch. In the minds of many, human finger touch still remains the only goddess of musical expression in pianoforte performance. The layman's denunciation that reproducing instruments are a bane to musical art because they lack the feeling power which human touch can render is not justified in the light of some simple laws of mechanics. Human finger touch is subject to the laws of mechanics like any other fact of nature, and the general notion that the artist is capable of transferring a personal magnetism to the keys is simply a notion that is engendered from ignorance of the facts. The musician, and especially the composer, in his feeling attitudes is ofttimes a mystic, and it is just the mystic who shuns any mechanistic conception of art despite the fact that he continually employs mechanical principles to serve his end. A momentary cross-section of any system of aesthetics is based upon a science of some form, and any science of form involves mechanical features. As Whipple (9) and Farnsworth (2) have shown, the reproducing instrument is capable of reenacting to a fine degree of precision the tonal fruits which the artist's finger touch has created in conformity with the mechanical principles of leverage action. Human finger touch has been accorded such a paramount position by the vast majority of listeners that any change in interpretation is likely to be attributed to change of this factor. The important rôle which the damper-pedal plays in expression is little recognized by the layman; yet the most flawless and perfect finger technique, unless supported by adequate pedalling (5), fails to achieve its purpose.

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LE RÔLE FONCTIONNEL DU DOIGTER ET DE L'USAGE DE LA PÉDALE FORTE DANS L'APPRÉCIATION DE LA MUSIQUE DU PIANO

(Résumé)

Le fait que les différences dans l'usage de la pédale forte du piano sont ordinairement aperçues comme différences dans le doigter est montré par les résultats obtenus d'une étude où il s'agit du jugement de 8 étudiants universitaires mâles connaissant la musique et 8 qui ne la connaissaient pas. On a demandé à ces sujets de comparer et de rapporter la nature de toutes les différences notées quand on a joué deux fois successivement la composition de Victor Herbert intitulée "Donne-moi encore un baiser" au moyen d'un piano méchanique Duo-Art. On l'a jouée d'abord comme indiquée. Ensuite on a éliminé les effets de la pédale forte tandis que tous les autres effets ont été les mêmes.

Quinze des 16 sujets ont déclaré que la variation du *doigter* a produit les grandes différences d'interprétation, bien que l'on n'eût pas changé le méchanisme du piano contrôlant le doigter. Aucun sujet n'a parlé de la pédale jusqu'à ce qu'on la lui ait fait remarquer. Ils ont considéré d'autres facteurs comme causes de variation: en ordre de fréquence, la durée tonale, le temps, le rythme, la phrase, l'accent, et l'intensité du ton. Bien que le temps, le rythme, et l'intensité du coup des marteaux aient été constants, ils ont pensé que le morceau joué sans pédale était d'un tempo plus rapide, plus accentué avec une plus grande intensité de ton, et plus semblable au rythme du fox-trot qu'à celui de la valse.

Heinlein

DIE FUNKTIONELLE ROLLE DES ANSCHLAGS UND DES PEDALS IN DER WÜRDIGUNG VON KLAVIERMUSIK

(Referat)

Dass Unterschiede im Gebrauch des Pedals beim Klavierspiel gewöhnlich als Unterschiede des Anschlags angesehen werden, erhellt aus den Ergebnissen einer Untersuchung die mit 8 musikalisch ausgebildeten Studenten und 8 Studenten ohne musikalische Bildung ausgeführt wurde. Den Versuchspersonen wurde Victor Herberts Komposition "Küsse mich noch einmal" zweimal hintereinander von einem Duo-Art Klavier vorgespielt, und beim zweiten Mal alle Pedalwirkungen ausgeschaltet. Nun musten die Vpn. Vergleiche anstellen und die Unterschiede zwischen den beiden Vorträgen sorgfältig notieren.

Fünfzehn der 16 Vpn. erklärten, dass Variation im Anschlag die Hauptunterschiede in der Interpretation verursacht hätte, obwohl der Mechanismus der den Anschlag kontrollirt, nicht verändert worden war. Keine der Vpn. erwähnte das Pedal, bis man sie schliesslich darauf aufmerksam machte. Andere Faktoren die erwähnt wurden sind in der Reihenfolge der Häufigkeit, Tonlänge, Takt, Rhythmus, melodische Gruppirung, Nachdruck und Tonstärke. Trotzdem dass Tempo, Rhythmus und Tonstärke unverändert blieben, so wähnte man doch bei dem Vortrag ohne Pedal ein schnelleres Tempo, mehr Nachdruck und grössere Tonstärke wahrzunehmen und meinte das Klavierstück sei dem Fox-Trot näher verwandt als dem Walzerrhythmus.

HEINLEIN

ФУНКЦИОНАЛЬНАЯ РОЛЬ ПАЛЬЦЕВОГО УДАРА И УПОТРЕБЛЕНИЯ ЗАГЛУШАЮЩЕЙ ПЕДАЛИ В ФОРТЕПЬЯННОМ ИСПОЛНЕНИИ.

(Реферат).

При опросе 8 музыкально-образованных и 8 музыкально-необразованных студентов колледжа оказалось, что изменения в фортепьянном звуке, происходящие от употребления заглушающей педали, обычно воспринимаются как изменения в качестве пальцевого удара. Испытуемые должны были сравнить и охарактеризовать все изменения отмеченные или при двух последовательных исполнениях пьесы Виктора Герберта „Kiss me Again", сыгранной на репродуцирующем рояле „Duo-Art". Первое исполнение следовало всем обычным правилам. При втором все детали исполнения остались те же, кроме левой педали, которая была уничтожена.

Пятнадцать из 16 испытуемых нашли, что главное различие в исполнении было вызвано изменениями пальцевого удара, несмотря на то, что механизм, контролирующий особенности удара, не был включен. Ни один из испытуемых не подумал о педали, пока их внимание не направили в эту сторону. Некоторые указывали на перемену темпа, звучания, ритма, фразировки, акцента и силы руки. Несмотря на то, что темп, ритм и интенсивность удара оставались неизмененными, испытуемые указывали, что исполнение без педали быстрей по темпу, более акцентировано, более интенсивно по звуку и по ритму ближе к фокстроту, чем к вальсу.

Гайнлайн (Heinlein).

AREA, COLOR, AND BRIGHTNESS DIFFERENCE IN A REVERSIBLE CONFIGURATION*

From the Psychological Laboratories of Clark University

CLARENCE H. GRAHAM

I

The literature on "figure and ground" is quite definite in its view that area is a very important factor in the perception of the phenomenon. Wever (4, p. 55) writes, in describing configurations which elicit most equivocal responses: "One of the factors in equivocality is equality of the areas of the two stimulus fields." Rubin (3) also recognizes this factor when he writes: "One can advance the following, fundamental law: If of two homogeneous, differently colored fields the one is noticeably larger than the other and surrounds the other, then there is an overwhelming probability that the small, inclosed field is perceived as figure."[1] Here he accepts color difference as a factor also.

This study is an attempt to get some quantitative measure of the influences of both area and color in a reversible configuration. The method is one of varying these factors in such a figure, measuring reversals, and analyzing the results. It is hoped that such a procedure will give a more definite, quantitative measure of the part that these factors play in configurations.

II

The figures used as stimuli in this experiment were similar to the drawing in Figure 1. This is a figure with which Köhler has worked, and is a modification of some of the figures used by Rubin, in that there are six sectors instead of eight. In certain ways this figure is better suited to the purpose than Rubin's. Since there are

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[1] "Man kann folgende fundamentale Regel aufstellen: Wenn von zwei homogene, verschiedenfarbigen Feldern das eine wesentlich grösser als das andere ist und das andere umschliesst, dann ist eine überwiegende Wahrscheinlichkeit dafür vorhanden, dass das kleine umschlossene Feld als Figur aufgefasst wird" (3, p. 79).

only six angles, the range of modification is from 0° to 120°. Rubin's range was 0° to 90°.

The figure was pasted in the center of a neutral gray card, 75 cm. by 55 cm., and consisted of a circle of Hering white, 15 cm. in diameter, divided into sectors by black lines drawn with India ink.

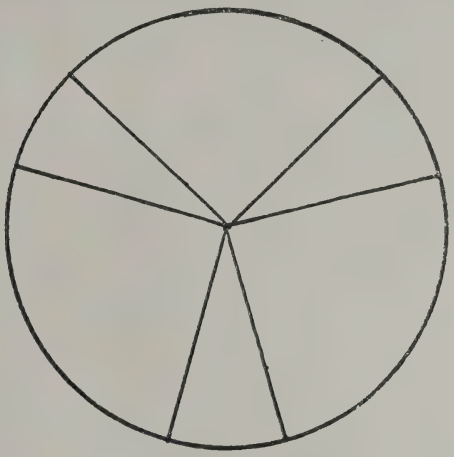

FIGURE 1
A 30° FIGURE

In rare cases the smaller sectors are not seen as "figure." The data of this experiment are selected in the sense that, if an observer reported eye-strain, too great confusion from after-images, etc., after a trial, that trial was thrown out.

In the colored series the sectors alternated, e.g., red, blue, red, blue, etc. The card with the figure on it was hung on the wall 230 cm. distant and on a level with *O*'s eyes. A 200-watt daylight bulb supplied the illumination. This was so placed that it was over and slightly in back of *O*'s head. The rays fell full on the figure, the room, of course, being brightly illuminated.

The apparatus for recording reversals consisted of a kymograph with the standard arrangement of telegraph key. Time was recorded by means of an electric clock in circuit with an electric marker. A chin-rest on a stand was provided for *O*. The telegraph key was placed on the stand in such a way as to be most easily manipulated.

The following observers served in these experiments: Dr. J. P. Nafe, under whose direction the problem was done, F. A. Geldard, and Miss D. E. Johannsen, both assistants in the department. These observers are designated respectively as N, G, and J.

Each observer was given at least one distributed hour of practice in the method of recording, so that when the experiment did begin

there was no uncertainty. *O* was instructed as follows: "You are to see this figure as a reversible configuration. Five seconds before the trial the experimenter will say 'All right.' Two seconds before the trial he will say, 'Ready,' and at the word 'Now' fixate the center of the figure (where the lines meet) and keep fixation throughout the entire trial. Press down when the small areas are 'figure' and release when the large are 'figure.'" Care was taken that instructions were thoroughly understood. Observations were with two eyes.

In the black-white series six figures were used. These figures were those in which the angles between the lines enclosing the smaller areas were respectively 10°, 20°, 30°, 40°, 50°, 60°.

In the blue-white series four figures were used. Blue sectors alternated with white sectors, the angles of the blue sectors being 30°, 45°, 60°, 90°. A black line was drawn in India ink along each contour for the purpose of making conditions as nearly like the black-white series as possible, with the exception of the factor of color. (All colors used were Hering.)

Each observation period consisted of at least 16 trials, and all averages are based on at least 16 cases. Usually only one figure was used during a period. Each trial lasted for 30 seconds, and all averages are in terms of time out of 30 seconds that a system of sectors was seen as figure.

III

In Table 1 the "Small" column shows the number of seconds out of 30 that the small sectors were seen as figure. The time for the large sectors as figure can, of course, be found by subtracting the time of the small from 30. The "Time" column designates the average time in seconds of each reversal measured from the appearance of Small as figure to the disappearance of Large as figure. "Per cent" column expresses the approximate percentage of total time that Small was figure.

A glance at Table 1 is sufficient to settle the question of the influence of area in reversals, for the black-white series at least. It is immediately seen that, as the area of the small system sectors decreases, the tendency is for them to be more stable as figure. Sectors with an angle of 10° dominate in reversals approximately one and one-half times as long as sectors which are equal, and the areas between vary in dominance (measured objectively in terms of time) in a liminal manner.

TABLE 1

RESULTS FOR THE BLACK-WHITE FIGURE

Observer	Angle	Small	A.D.	Time	Per cent
N	10°	21.4	1.1	5.4	71
	20°	20.6	1.2	4.6	69
	30°	19.8	0.9	4.4	66
	40°	17.6	1.0	4.4	59
	50°	16.7	1.3	4.0	56
	60°	15.1	0.6	3.5	50
G	10°	23.1	0.9	3.6	77
	20°	21.9	0.8	3.5	73
	30°	19.6	1.0	3.3	65
	40°	18.3	1.0	4.3	61
	50°	17.7	0.8	2.5	59
	60°	15.0	—	2.1	50
J	10°	22.0	0.8	3.3	73
	20°	18.2	1.4	3.7	61
	30°	17.6	1.5	4.1	59
	40°	16.2	0.8	5.2	54
	50°	15.7	1.2	7.5	52
	60°	15.1	0.8	7.0	50

Is there a rhythm for reversals? We can answer in the affirmative, but with reservations. The most apparent rhythms occurred for N and G at angles of 60°. The comparatively small average deviations for both observers give evidence of this. G reported a perfect rhythm. He reported that the sectors "came in and went out like the beat of a metronome." He further insisted that he was careful not to let a rhythm creep in. N also found a rhythm, although it was not so marked as G's. He reported that "he could almost feel the pulsing of the figure," and for long periods his records show this. However, now and then something would seem to break up the rhythm.

In the beginning of the experiment, trials were one minute in length, but so many factors (such as eye-strain) entered into the situation that the trials were cut to 30 seconds. A continual source of difficulty after the first 20 seconds of the trial was what Ferree (1, p. 114 ff.) has called the streaming phenomenon. At moments in the last 10 seconds, part of the lines or even whole lines were blotted out, but in giving reports all of the observers said that regardless of this they were always sure of a change in the figure. Even though the contours were blotted out, the luminosity and areal distinctions usually remained. There could be no question as to when

a change occurred even under these conditions. We draw no conclusions from the phenomenology of the experiment.

IV

In the experiments with blue the results are more ambiguous. In order to get a clear idea of what happens, it is better to consider each observer separately.

Observer G. Table 2 gives a clear idea of G's results. For him blue adds to the dominance of figure, his results showing that the colored sectors dominate always slightly more than do white sectors of the same area. Furthermore, his results are in harmony with his results in the black-white series, showing the same gradations.

Observer N. Table 2 gives the results of observer N. It is apparent that there is not much consistency with former results. In the blue-white series he does not show the trend which he showed in the black-white series. The time of reversals is practically constant, and although there may be a sort of order in his results from 45° up to 90°, the results for the 30° angle are entirely inconsistent with his former results. He reported that he was never certain of a reversal, and, in fact, that if there were any reversals they were very indefinite. Mists and hazes were much more bothersome than in the black-white series. However, he was definitely certain that for him white was dominant.

TABLE 2

RESULTS FOR THE BLUE-WHITE FIGURE

Observer	Angle	Blue	A.D.	Time	Per cent
N	30°	13.4	1.2	4.1	45
	45°	16.5	1.7	4.3	55
	60°	14.8	1.3	4.2	49
	90°	10.8	1.3	4.0	36
G	30°	23.5	1.2	4.2	78
	45°	21.6	0.6	3.1	72
	60°	16.1	1.4	2.0	54
	90°	7.0	1.0	3.2	23
J	30°	10.6	3.2	4.0	35
	45°	14.2	1.2	5.4	47
	60°	7.0	0.9	5.8	23
	90°	12.4	1.3	5.7	41

Observer J. Table 2 gives the results for J. J reported that she could not get figure and ground consistently. Sometimes white would stand out, but blue hardly ever. Usually the sectors did not form a figure-ground configuration, but seemed to be blue and white sectors in the same plane. Sometimes the white would stand out, but the blue would appear only as sectors in back of the white, not as ground. J was certain of only one thing, and that was that white was more dominant than blue.

From this part of the experiment we here draw no definite conclusions. The uncertainty of interpretation of N's and J's results makes it difficult to do this. If their results are to be accepted, it would seem that there are great individual differences in figure-ground experience.

V

A series of three red and blue figures was used with G. The angles of the red sectors were 30°, 45°, and 60°. Table 3 expresses the results obtained. For G it is seen that in this case the 30° and 45° angles are much more dominant than in the black-white figure. It would seem at first glance as though the increase in dominance must be due to color differences. Furthermore, G reported that red was more dominant. The 60° figure is an exception. The results and the report show that blue is about as dominant as red. However, this figure was presented along with the blue-white figures. It is easy to guess that the observations in the blue-white series carried over a set for this configuration.

At this point the method was changed. From this point on, observations were made monocularly, and with an artificial pupil of 2 mm. diameter. This change was made when it was found that by this method the mists and hazes which the *O*'s had been reporting

TABLE 3

RESULTS FOR THE BLUE-RED FIGURE

Observer	Angle	Red	A.D.	Time	Per cent
G	30°	24.2	0.8	8.3	81
	45°	20.2	1.5	4.8	67
	60°	14.0	0.9	2.2	47
N	30°	21.2	0.6	6.8	71
(with pupil)	45°	18.0	0.5	4.4	60
	60°	15.8	0.6	3.5	53

were eliminated to a large extent. All the *O*'s reported that judgments were much easier when the artificial pupil was used. A few observations showed that there was no marked difference produced in the results obtained by this method when compared with the results obtained by the former method. However, pupil area, as a possible factor, was controlled.

N's results with the red-blue series are also presented in Table 3. His results also show a slight tendency for the red to dominate.

The ambiguous results obtained from G and J in the blue-white series left us without any conclusions as to the influence of color in this figure. Such results would seem to show uncontrolled factors. The type of reports given and the results obtained led the writer to the belief that brightness differences might be a very important factor. The literature quite generally stresses this possibility. This may have been also a factor in the red-blue series.

It was decided to test the effect of brightness difference, using a modification of the red-blue figure. In this case the brightnesses were equated.

The figure was constructed of gelatine filters cut in the shape of the desired angles. The differently colored gelatines were carefully fitted together and placed upon a square plate of ground glass, 31 cm. by 31 cm. The angular gelatines were kept in place by making them much larger in radius than necessary for the figure, and then by fastening the portions outside the area required for the configuration with gummed paper. A square of black velvet paper (the same size as the glass plates), the center of which had been cut out in the form of a circle of the same size as the black-white series (15 cm. in diameter), was placed over the gelatines. Over this was placed a square of ordinary glass of the same size. The piece of apparatus thus made was merely a complex color filter, giving by means of transmitted light the same type of configuration as was obtained with the pigment colors.

The equating of the brightnesses of the red and blue was done by the flicker method. It was hoped that by using a large enough number of red gelatines to one or two thicknesses of blue that the brightnesses could be equated. However, the number of red filters necessary was so large that a piece of ordinary typewriting paper was used also. The two colors were equated when the ratio was seven red gelatines and one piece of typewriting paper to one blue gelatine. (The white paper lessened the saturation of the red to some extent.)

The apparatus of which this complex filter was a part consisted of a large light-tight box, 100 cm. in length, 35 cm. in width, and 35 cm. in height. The inside was painted black. The front end of the box was made with a square opening so that the figure in the glass plates could be placed over it exactly. A 50-watt light in the box supplied the illumination at a distance of 50 cm. behind the figure. Thus, when the *O* looked at the stimulus in a dark room, he saw a translucent screen of exactly the same dimensions and of exactly the same appearance as the figures which he had been observing formerly, the only difference being that the brightnesses of the red and blue were equal. The brightness of the observed field was 16 millilamberts.

Observations were made in a dark room. The *O* was seated at a distance of 160 cm. from the stimulus. All of the other conditions were the same as in previous experiments. Observations were for 20 seconds. Fifteen trials were given to each figure. At this point M. N. Crook (C) was substituted for G, G no longer being a member of the department.

Before the series with equated brightnesses, a few observations were made with J and C with figures of unequal brightness. This was done in order to get a more satisfactory comparison than G's and N's results with pigments would give. Blue in this case was figure as compared with the series with G and N, in which red was figure. In this case the red was made brighter by simply taking out the white paper from behind the red gelatines.

The results for both *O*'s are given in Table 4. It is apparent that with brightnesses unequal the influence of area is greatly affected. In the case of the 30° blue angles (in which opportunity is very favorable for blue to dominate) blue dominates less than half the time, and in the case of the 60° angles, when, according to the conditions of area, the figure should be most equivocal, the red dominates.

TABLE 4

RESULTS FOR THE BLUE-RED FIGURE (RED BRIGHTER—TRANSMITTED LIGHT)

Observer	Angle	Blue	A.D.	Time	Per cent
J	30°	7.6	1.6	5.5	38
	60°	6.2	0.4	3.4	31
C	30°	9.6	0.9	3.4	48
	60°	8.3	1.3	3.8	42

Let us in this connection refer again to Table 3. This gives N's and G's results for the red-blue pigment figures. It was pointed out in discussing their results that there is a slight tendency for red to dominate, a fact which is shown much more clearly by C's and J's results with this figure.

Table 5 gives N's, J's, and C's results when the brightnesses are equated. It is obvious that the results of the three *O*'s are consistent with respect to percentage. J's values are lower than the values for the other two, but when these results are compared with the results in the series of unequal brightnesses a very definite trend is shown, i.e., when "figure" and "ground" are of equal brightness, area is a most important factor in the perception. The smaller one field is in relation to another, the longer it will dominate in reversals. This seems to be the case even when "figure" and "ground" differ in color. When they are of unequal brightness, the influence of area is enhanced or overcome, depending on whether the larger or smaller area is brighter.

There seems to be an exception to this. G's results with the blue-white figure show the same trend as his results for the black-white figure. This is opposed to the majority of cases. We cannot explain it except as a difference in set. Certainly, in this type of experiment, set is a factor which should be controlled rigidly. Passivity, above all else, should be stressed. Figures used should be divorced as far as possible from the past history of the subject. For this reason equivocal figures presenting black and white areas would

TABLE 5

RESULTS FOR THE BLUE-RED FIGURE (BRIGHTNESS EQUATED—TRANSMITTED LIGHT)

Observer	Angle	Blue	A.D.	Time	Per cent
N	30°	12.1	1.6	7.0	61
(Left eye)	45°	11.4	1.7	7.8	57
	60°	10.0	1.7	9.2	50
	90°	6.6	1.4	7.2	33
C	30°	12.3	1.2	2.9	62
(Left eye)	45°	10.5	0.8	2.7	53
	60°	9.8	0.9	3.1	49
	90°	6.1	0.7	3.1	31
J	30°	10.4	2.2	6.4	52
(Left eye)	60°	8.6	1.9	5.1	43

not seem to be good, for the subject's past experience with printed material, etc., would create a set for the black as "figure."

VII

In order to check further on these results, a red-green figure was used. The provision of a new figure was made in order to see whether the trend shown with the other figures would apply in this case. Two more *O*'s were used, Miss L. M. Harden and N. L. Munn (H and M), for the purpose of recording the responses of two *O*'s who were completely unacquainted with the problem.

The ratio of green gelatines to red was 2 to 1. The brightness of the observed field was 94 millilamberts.

Table 6 gives the results of all five *O*'s for this series. It is seen that the results are all consistent, in so far as ratio of dominance is concerned. The two new *O*'s give results comparable to those of the others. This series merely substantiates the fact brought out in the last series, that when in a configuration, "figure" and "ground" are of equal brightness, relative area is a most important factor.

VIII

We have not considered as yet the facts concerned with the period of reversal—those notations listed under the column marked "Time."

TABLE 6

RESULTS FOR THE RED-GREEN FIGURE (BRIGHTNESS EQUATED—TRANSMITTED LIGHT)

Observer	Angle	Red	A.D.	Time	Per cent
N	30°	13.4	0.8	3.7	67
(Left eye)	45°	11.8	1.1	4.9	59
	60°	10.5	1.3	8.7	53
C	30°	12.3	0.6	2.6	62
(Right eye)	45°	11.3	0.7	2.8	57
	60°	10.1	0.4	2.4	51
J	30°	11.6	1.4	5.4	58
(Left eye)	45°	10.6	1.1	3.3	53
	60°	9.0	1.8	4.8	45
H	30°	13.0	1.9	8.8	65
(Left eye)	45°	10.6	1.2	4.0	53
	60°	9.7	1.7	4.7	49
M	30°	12.3	1.2	8.5	62
(Right eye)	45°	11.4	1.0	6.6	57
	60°	10.2	1.1	5.0	51

It was mentioned in commenting on the results with the black-white series that there was a rhythm for G with the 60° angle, and a rather definite rhythm for N for the same figure. These results are the only ones that show such a tendency.

In analyzing such data, one gets pretty close to the work done on the problem of the so-called "fluctuations of attention." Guilford (2), working with weak visual stimuli, found that "fluctuations are most rapid at a midpoint which is near the limen for fluctuations and become less frequent as the stimulus is made more or less intense."

In the figure used here, there can be no variation in the intensity of the figure, as the areas of the sectors are varied. However, there would seem to be something like a liminal gradation in the response as the areas are changed, the ratio of dominance of one system of sectors to the other being one to one when the areas are equal. As the areas are varied with relation to each other, the dominance measured in time varies.

Is there any similar definite trend for the periods of reversal? With N and G in the black-white series, we find such a trend. Fluctuations are more rapid at the 60° angle and become less frequent as the angle gets smaller. J's results are opposed to these. M and H show results with the red-green figure, which are comparable with N's and G's results in the black-white series. In the red-blue and red-green series, N gives results which are opposed to his former results. The greatest time of fluctuation occurs at 60°. J, with both the red-green and blue-red figures, shows a slight tendency for reversals to occur more rapidly at 60°. C's periods of reversals do not vary definitely in any direction.

Such results as these show nothing definite. They can be explained only when all the facts of the so-called "fluctuations of attention" are presented.

Another problem which presents itself is the problem of the influence of color. We have not done very much with this. Certainly the results would seem to show that color is not a very great factor in the determination of the ratio of the dominance of one system of sectors to another. However, there may be some likelihood that color is an important factor in the period of reversal. The question of the relation of the sensory intensity of the stimulus and the phenomenon of reversal has not been touched. These problems might be studied with indicative results.

Summary

In this problem we studied the relative contributions of area and color (with and without equating for brightness) to dominance of "figure." The figure used was a suitable reversible configuration. Kymograph records of reversals were obtained. The criterion of dominance was the length of time (out of a definite period) one system of fields was seen as "figure" as compared to the length of time the other system of fields was seen as "figure." By varying the areas of the two systems, by introducing color, and by equating the colors for brightness, we obtained a quantitative expression of the influence of area, color, and brightness differences in reversals.

Our results show that in this type of configuration (1) the system of smaller fields is chosen as "figure," and a relative decrease in the size of these fields makes them more dominant, i.e., they persist as "figure" longer in reversals. Dominance varies inversely in a liminal manner with area. When the systems are equal in area, the configuration is most equivocal. (2) When "figure" and "ground" are of equal brightness, area is a most important factor in the perception. This seems to be the case, regardless of color differences. (3) When "figure" and "ground" are not of equal brightness, the influence of area is greatly affected, and, if the brightness differences are great enough, the tendency is for the brighter system to dominate. (4) There is no indicative tendency shown in the length of the period (or frequency) of reversal.

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LA DIFFÉRENCE DE SUPERFICIE, DE COULEUR, ET DE CLARTÉ DANS UNE CONFIGURATION RENVERSABLE

(Résumé)

Dans ce problème il s'agit des contributions relatives de la superficie et de la couleur (avec et sans rendre égale la clarté) à la dominance de "figure." La figure employée a été l'adaptation d'une des figures de Rubin par Köhler. On a obtenu des notations des renversements par le kymographion. Le critérium de dominance a été la durée relative dans une période définie pendant laquelle on a vu un système de champs comme "figure" comparée à la durée relative pendant laquelle on a vu l'autre système comme "figure." Au moyen de varier la superficie des deux systèmes, d'employer la couleur, et de rendre égale la clarté des couleurs, on a obtenu une expression quantitative de l'influence des différences de superficie, de couleur, et de clarté dans les renversements.

Les résultats montrent que dans ce type de configuration (1) le système des plus petits champs est choisi comme "figure," et une décroissance relative de la grandeur de ces champs les rend plus dominants. La dominance varie inversement d'une façon liminale avec la superficie. Quand les systèmes ont la même superficie, la configuration est le plus équivoque. (2) Quand la "figure" et "surface" ont la même clarté, la superficie est un facteur très important de la perception. Cela semble vrai malgré les différences de couleur. (3) Quand la "figure" et la "surface" n'ont pas la même clarté, l'influence de la superficie est beaucoup influencée, et si les différences de clarté sont assez grandes, le système de plus grande clarté tend à dominer. (4) Nulle tendance indicative ne se montre dans la durée de la période (ou fréquence) de renversement.

GRAHAM

UNTERSCHIEDE DES FLÄCHENINHALTS DER FARBE UND HELLIGKEIT BEI EINER UMKEHRBAREN GESTALT

(Referat)

Bei diesem Problem handelt es sich um die verhältnismässige Bedeutung des Flächeninhalts und der Farbe (mit und ohne Rücksicht auf Helligkeit) bei dem Hauptfaktor, "Gestalt." Die Gestalt war Köhlers Anpassung einer Figur Rubins. Man machte Platten der Umkehrungen mit einem Kymographen. Der Massstab des Hauptfaktors war der verhältnismässige Zeitraum einer bestimmten Periode während dem ein System von Feldern als "Gestalt" gesehen wurde, im Vergleich zu dem verhältnismässigen Zeitraum während dem das andere System als "Gestalt" gesehen wurde. Durch Veränderung des Flächeninhalts der beiden Systeme, durch Einschaltung von Farbe und durch die Gleichung der Helligkeit, erhielt man ein quantitatives Mass des Einflusses der Unterschiede des Flächeninhalts sowie der Farbe und Helligkeit bei Umkehrungen.

Die Ergebnisse zeigen dass bei dieser Art der Umgestaltung (1) Das System kleinerer Felder als "Gestalt" gewählt wird, und die relative Verkleinerung dieser Felder sie stärker hervortreten lässt. Der Hauptfaktor variiert umgekehrt schwellenartig mit dem Flächeninhalt. Wenn die Systeme den gleichen Flächeninhalt haben, so ist die Gestaltung am zweifel-

haftesten. (2) Wenn Gestalt und Hintergrund von gleicher Helligkeit sind, so ist der Flächeninhalt der wichtigste Faktor in der Wahrnehmung. Dies scheint der Fall ganz abgesehen von Farbenunterschieden. (3) Wenn "Gestalt" und "Hintergrund" nicht die gleiche Helligkeit haben, so wird der Einfluss des Flächeninhalts stark afficirt, und wenn die Helligkeitsunterschiede bedeutend genug sind so wird wohl das hellere System vorherrschend wirken. (4) Man fand keine Anzeichen in der Länge der Periode (oder Häufigkeit) der Umkehrung.

GRAHAM

РАЗЛИЧИЯ В ПОВЕРХНОСТИ, ЦВЕТЕ И ЯРКОСТИ ПРИ НЕУСТОЙЧИВЫХ КОНФИГУРАЦИЯХ.

(Реферат).

Эта проблема рассматривается с точки зрения отношения поверхности и цвета (с приравнением и без приравнения к яркости) к доминированию „фигуры". Употреблялась одна из фигур Рубина, проверенная Келером. Были получены кимографические записи смены конфигураций. Критерием доминантности являлась относительная длина времени определенного периода, когда одна система воспринималась как „фигура", по сравнению с относительной длиной времени, когда за „фигуру" принималась другая система. Варируя поверхности двух систем, вводя цвет и уравнивая цвета в отношении яркости, было найдено количественное выражение влияния различий в поверхности, цвете и яркости на смену конфигураций.

Результаты показали, что при этом типе конфигурации 1) за „фигуру" принимается система меньших поверхностей, и постепенное уменьшение величины этих поверхностей увеличивает их доминантность. Изменение доминантности обратно-пропорционально изменению поверхности. Когда системы разны по поверхности, конфигурация сомнительна. 2) Когда „фигура" и „фон" равны по яркости, то поверхность является важнейшим фактором восприятия. Это не меняется при перемене цвета. 3) Когда „фигура" и „фон" не одинаковы по яркости, то влияние фактора поверхности уменьшается; при достаточно сильной разнице в яркости доминирует наиболее яркая система. 4) В отношении длины периода доминирования конфигурации никакой определенной тенденции не наблюдается.

Грехэм (Graham).

GEOTROPIC ORIENTATION IN ARTHROPODS: IV. THE BEETLE *PYROPHORUS**

From the Laboratory of General Physiology and the Zoölogical Laboratory, Harvard University

BENJAMIN KROPP

I

The manner and explanation of geotropic orientation in a variety of animals have been described in detail (3, 4, 5, 6, 9, 8). For insects (caterpillars, beetles), Crozier and Stier (5, 6) have been able, by reference to the mechanics of simple creeping, to formulate mathematical expressions for the respective types of response. This had previously been accomplished for rats (4) and mice (2, 7), but the mechanical differences in modes of support and progression between mammals on the one hand and arthropods on the other involve necessarily different formulae for their expression (1). The essential factor is the differential pull on the leg and body muscles of each side as altered by change of position of the animal's center of gravity with relation to the angle of inclination of the creeping plane, while the animal is in motion (3).

In the case of arthropods, as indeed with rats and mice, it must be assumed that the orientation of the animal during progression, as manifested in its creeping path, is an expression of the sensible equality of impressed muscular tensions on each side of the animal (5, 6).

II

The elaterid beetle *Pyrophorus noctiluca*[1] is especially well adapted for investigations of this sort, since it is possible to rule out entirely the factor of light. This is of considerable importance, since the amount of light adequate to stimulate is very small. Even in the presence of a red "safe" light, the animals were photopositive.

*Received for publication by W. J. Crozier of the Editorial Board, August 27, 1928.

[1]We are greatly indebted to Mr. Robert M. Gray, Superintendent of the Harvard Biological Laboratory, Soledad, Cienfuegos, Cuba, for kindly supplying us with these animals.

TABLE 1

MEAN ANGLES OF UPWARD ORIENTATION OF FIVE *Pyrophorus noctiluca* DURING CREEPING UPON A PLANE INCLINED AT VARIOUS ANGLES (α) TO THE HORIZONTAL

The values of θ are each the mean of 25 trails, five on each of five individuals.

α	Θ	P.E. θ as per cent mean θ
10°	27.4°±1.36°	5.95
20°	42.2°±0.79°	1.87
25°	48.0°±0.60°	1.26
30°	54.1°±1.90°	3.52
35°	61.8°±0.86°	1.39
40°	68.6°±0.63°	0.92
45°	75.5°±1.01°	1.33
50°	82.3°±0.56°	0.56

Tetraopes (6) is usually geotropically active only in the light. *Pyrophorus* possesses two large luminous organs on the dorsal surface of the head, one near each lateral posterior margin. During creeping these organs are usually luminescent, or, if not, a gentle tap on the elytra will induce it. The light produced is more than sufficient to permit following the orientation path with great ease. A straight edge was placed parallel to the path of creeping while the animal was in motion, and the angle of upward orientation on the plane, θ (5), measured with a protractor. In this way the entire path of progression was observed for a distance of 40 cm. The animals were started from a horizontal position near the base of the creeping plane. As emphasized in several previous cases, excessive handling invariably causes loss of ability to orient, and gives inconsistent paths and nonsensical results. To avoid all but the necessary modicum of handling, the animals were permitted to creep about for a few minutes on the creeping surface confined under a petri dish, before a trail was begun and recorded.

The animals crept on black velvet spread smoothly over a flat surface. Velvet was found the most favorable surface for use with these animals, since the soft nap allowed proper "gripping" without injury to the tarsal claws, an important point for normal locomotion. The individuals used for tests must be carefully chosen with regard to the normal condition of the legs and feet. The upper limit at which readings could be taken was $\alpha=50°$; above that inclination the animals fell off the creeping plane. Below $\alpha=10°$, readings were not consistent.

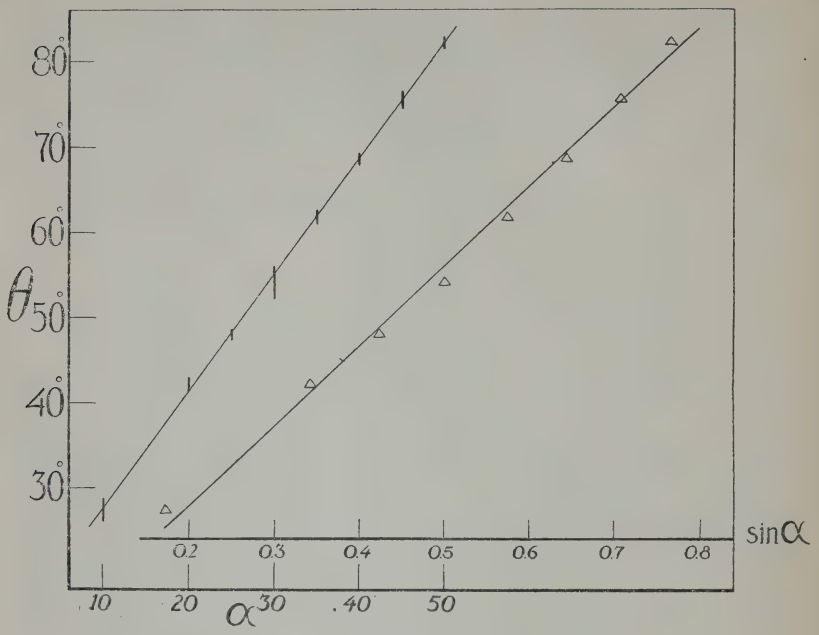

FIGURE 1

MEAN ANGLES OF UPWARD ORIENTATION OF FIVE *Pyrophorus* DURING CREEPING UPON A PLANE INCLINED AT ANGLES α=10° TO 50°, INCLUSIVE, TO THE HORIZONTAL

The center of each plotted bar indicates the mean of twenty-five trails; the height is equal to 2 P.E. Plotted triangles indicate the direct proportionality to the angle of orientation θ with sin α

III

As with the beetle *Tetraopes* (6), and the fiddler crab *Uca* (8)—although for different reasons—the average angle of orientation θ is directly proportional to the sine of the angle α, $\theta = K \sin \alpha + C$. This relationship is shown in Figure 1. All the points fall on a straight line within the limits of the probable errors of the means. Table 1 and Figure 1 give the results in summary. Within the short range of values of α available in the present case, θ is linearly proportional to α more closely than to sin α. The uniformity of results is so apparent that there can be no doubt of the homogeneity

of the data and of the absence of gross defects in procedure.[2] As in the case of other arthropods, the probable errors of θ decline with sin α (Table 1).

Summary

The geotropic orientation of the luminous tropical beetle *Pyrophorus* obeys the equation found for *Tetraopes*. The angle of upward, oriented creeping is directly proportional to the sine of the inclination of the surface. The mechanics of geotropic response is the same in these cases, the different slopes of the lines in the two cases indicating only respectively different constants.

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[2]It might be objected that the animal is stimulated by its own light. This is undoubtedly not the case, since the eyes are at the anterior part of the head and separated from the luminous organs by the vault of the head. Light from the luminous organs is thrown to either side of the head and downward, but the eyes are effectively shaded by the curved crest. No readings of normal creepings when the luminous organs were inactive were recorded, but careful observation showed satisfactorily that the angle of orientation θ was the same for a given value of α whether these organs were luminous or not, and hence exerted no photokinetic effect upon orientation. The animal is certainly not stimulated by its own light.

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L'ORIENTATION GÉOTROPIQUE CHEZ LES ARTHOPODES. IV. LE SCARABÉE PYROPHORE

(Résumé)

L'orientation géotropique du scarabée luminineux des tropiques *Pyrophore* obéit à l'equation trouvée pour *Tetraopes*. L'angle du rampement orienté vers un lieu plus élevé est directement en proportion avec le sinus de l'inclination de la surface. La méchanique de la réponse géotropique est la même dans ces cas, les différentes pentes des lignes dans les deux cas indiquant seulement des constants respectivement différents.

KROPP

GEOTROPISCHE ORIENTATION DER ARTHROPODEN: IV. DER KÄFER PYROPHORUS

(Referat)

Die geotropische Orientation des tropischen Leuchtkäfers *Pyrophorous* folgt der Gleichung die man für *Tetraopes* fand. Der Winkel des aufwärts gerichteten Kriechens steht in direktem Verhältnis zu dem Sinus Inklination der Fläche. Die Mechanik der geotropischen Reaktion ist in diesen Fällen dieselbe, indem die verschiedenen Inklinationen der Linien in beiden Fällen nur beziehungsweise verschiedene Konstante andeuten.

KROPP

ГЕОТРОПИЗМ У ЧЛЕНИСТОНОГИХ. IV. ЖУК PYROPHORUS.

(Реферат).

Геотропическая реакция светящегося тропического жука Pyrophorus (из сем. Elateridae — щелкунов) подчиняется уравнению, найденному для Tetraopes. Угол под'ема, взятый по отношению к направлению движения, прямо пропорционален наклону поверхности. Механизм геотропической реакции в одинаковых случаях один и тот же, различия в наклоне линий в двух случаях указывают лишь на соответственные различия констант.

Кропп (Kropp).

A DISCUSSION OF THE NATURE OF PIANOFORTE DAMPER-PEDALLING TOGETHER WITH AN EXPERIMENTAL STUDY OF SOME INDIVIDUAL DIFFERENCES IN PEDAL PERFORMANCE*[1]

From the Psychological Laboratories of The Johns Hopkins University

Christian Paul Heinlein

The story is often related how Safonof, the eminent Russian pianist, emphasized to his pupils the ideally refined use of the pianoforte damper-pedal as a vital means of artistic expression. We are told by Scholes (5) in *Crochets* how Safonof would draw the attention of his whole class to Scriabin's pedalling, telling his pupils not to look at Scriabin's hands but at his feet. "Like Sasha's pedalling!" was Safonof's highest compliment to a pupil. But not many teachers of the pianoforte have taken such a vital interest in the science of damper-pedalling.

It may surprise the reader to learn that pianoforte students in many conservatories of supposedly A-1 standing are ordinarily not taught how to use the pedals; they are simply allowed to drift and are expected by their tutors to pick up for themselves this most subtle art. The question immediately arises: Why are teachers of the pianoforte so lax with any explanation of the use of the pedal? The answer is, probably, a simple one; in all likelihood the majority of teachers do not understand what principles they themselves make use of in pedalling. Eighteen college students, who have had more than four years of pianoforte training, were questioned about their acquisition of pedal control, and in every case except one the students were allowed by their tutors to invent their own rules of pedalling.

In many instances, both the professional pianist and the amateur performer pedal on the whole without any forethought to a system of principles or rules. Some students, who have acquired excellent finger technique, are often deficient in their use of the damper-pedal.

*Received for publication by Knight Dunlap of the Editorial Board, September 25, 1928.

[1]The investigation recorded in this paper was carried on during the tenure of a National Research Fellowship in the Biological Sciences.

Sometimes the pedal is ignored altogether, while at other times, irrespective of the actual harmonic and melodic outline, it is depressed mechanically at the beginning of each measure. Some students pedal vaguely every now and then, when the fingers are not busily occupied with the keyboard, and a little attention can be directed to the feet. Still other students often use the pedal as a means of covering up gross blunders and inadequate finger technique. Most teachers are really stumped by the complexity of the problem, especially since so few books have been written on the subject, and since the amount of space devoted to it in most works on pianoforte playing is so meager. Descriptive studies for the most part have proved grievously inadequate and incomplete, while little attempt has been made to indicate anything beyond a few broad principles of pedalling.

The writer's purpose in this paper does not include any intention of presenting an elaborate discourse on the systematic use of the damper-pedal. No serious attempt will be made to enlighten the misguided pupil in order to help him retrace his steps into a vista of stereotyped rules. Fortunately, musical art is not stereotyped. Musical art would, indeed, suffer most intensely if interpretation were restricted to an exact and mechanical outline. That renowned pianists do interpret a given musical composition differently is a recognized fact, and precious few are the pedagogues who will dare state that a particular interpretation is the *only* permissible interpretation. Spontaneity, a salient emotional characteristic of the musician, is indeed one of the chief charms of interpretation. An attempt will be made herein to justify, within certain reasonably acceptable limits, the function of variability in musical interpretation, and to establish beyond a question of doubt the damper-pedal as an important, indispensable means and device for expressing individuality of taste in the interpretation of a given musical composition (2).

It is not amiss in this connection to call attention to the fact that there exist two schools of pedalling with divergent and antagonistic aims. The first school, far in the minority, insists that a single definite pedal pattern is the one and only correct one to employ in a given musical phrase, while the second school believes that a given musical phrase may be subjected to several different pedal patterns with satisfactory results, provided that these several different patterns do not flauntingly violate the bedrock principles of simple and effective pedal technique. A brief study of the history of pianoforte

interpretation makes acceptance of the views of the first school of pedalling exceedingly difficult.

Yet despite this fact that it is hard to establish fixed and unchanging principles of pedalling, one must not assume for a moment that there are no real technical difficulties in pianoforte pedalling. Any pedal pattern may not be a relevant one for a given passage. The ear-splitting errors in which so many students indulge is sufficient proof of this fact. Although the declaration may be outrageously dogmatic that a certain pedal pattern is the one right and only right one to use in a given musical phrase, it is not, however, improper to refer to a pedal pattern for a given phrase as being either *appropriate* or *inappropriate* as the case may be.

The persistent prevalence of sympathetic intervals in all music played with undamped strings makes clear the function of the damper-pedal. The damper-pedal, if discreetly employed, is a positive aid in surrounding the music with an atmosphere of appropriate harmonics and overtones. While these generated partials are not perceived as segregated, individual tones, they nevertheless serve to enrich the musical outline and to add a colorful background. This richness in color of tonal background is not always desirable. In a sequence of passing notes, for example, any prolonged sustension of the damper-pedal is superfluous and functions to obscure the melodic contour. Then, too, undue depression of the damper-pedal may lead to the unblending dissonance of superposed antagonistic harmonies.

In regard to the adequate use of the damper-pedal, a number of principles can be mentioned to which, however, no strict and rigid adherence can be maintained. It is desirable, for example, to depress the damper-pedal when the music is so naked in outline that it seems to need tonal support and a fullness of volume. It is a safe principle to change the pedal with a marked change in harmony, such as a progression from dominant to tonic or subdominant to tonic, although even this rule is not observed in instances when the triad form is played diatonically or chromatically in rapid succession for the deliberate purpose of obscuring the harmonic background. In instances where the sustension of the harmony more than compensates for the disadvantage of allowing conflicting tones to vibrate concurrently, the damper-pedal may be depressed. The damper-pedal may ofttimes be depressed to advantage when fortissimo effects are desired (although this is not the chief mission of the pedal, as is commonly supposed); when legato effects are desired; when the melodic scheme

coincides with the harmonic background; in broken chords; in arpeggios; in repeated chords; in sustaining the root of a given harmonic, melodic, or rhythmic phrase; in sustaining the root as melody when supported by a treble harmonic accompaniment; in an accompaniment with arpeggio or chordal repetition; and in silent pressure.

It is desirable to release the damper-pedal in passages where passing notes would be likely to function as disturbing factors; in the repetition of arpeggios and chords in the lower pitch registers; in passages where the harmonic atmosphere becomes dense and obscure with conflicting dissonances; when the bass is chromatic in character; when staccato effects are desired; when chords and arpeggios are repeated in the lower pitch registers; in introducing unaccompanied themes where melodic clearness is most desirable; in the case of detached chords; in contrapuntal music and in fugues. Many more such principles might be listed, but additions would have to be tentative only. The list given above constitutes principles which are recognized as fundamental in effective pedal technique.

One might be led to believe that following a survey of such a lengthy list of rules, the use of the damper-pedal becomes clearly defined. However, this is not true, since any musician will point to numerous instances in which violation of these rules is not only possible but highly desirable and proper. The confusion increases manifold, and any simple solution to the whole problem appears more distant than ever when one turns to the masters of the pianoforte for assistance. Indeed, one of the most serious reasons for the neglect to define more accurately where the damper-pedal is to be used is that the greatest virtuosos do not agree among themselves as to where or where not a particular pedal effect should be introduced. For a given musical phrase, one artist will insist upon rhythmic pedalling, another upon melodic pedalling, while a third will resort to harmonic pedalling. Which of these several interpretations can we consider as the "correct" one?

Since the masters of any art are the greatest authorities of that art, the writer has directed his attention to the pianoforte performances of a few world-famous virtuosos for enlightment and assistance in the process of formulating a feasible hypothesis. Duo-Art[2] repro-

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[2]The author wishes to express his thanks and appreciation to the Aeolian Company of New York City for their kind cooperation and generous loan of the Duo-Art reproducing piano which made possible this experimentation. He has also received special permission from the Aeolian Company to publish kymographic records of the reproducing rolls included herein. For an evaluation of the Duo-Art as a laboratory instrument, see (1).

ducing piano records of renditions by Ethel Leginska, Leopold Godowsky, Harold Bauer, and Joseph Hofmann were employed in an analysis of the damper-pedal interpretations of some very familiar compositions. Individual pedal patterns were recorded in picture wave form on a continuous smoke-drum kymograph, which was connected through a series of pulleys to the damper mechanism of the recording piano. This method of studying a specific pianoforte function has many advantages over the method of direct roll analysis such as Whipple (6) has employed. The kymogram presents the pedal performance in the form of a wave pattern, which indicates at a glance the actual characteristics of the pedal depression in terms of incidence, protensity, and amplitude.[3] In a statistical analysis of pedal pattern, the incidence and protensity of depression and release are related to the time factor in seconds and to the pitch points of the melodic contour. These latter two functions are recorded on the kymograph by means of electromagnetic pens. The time pen is connected in circuit with a seconds chronometer, while the melody pen is connected in circuit with a special electric recording device attached to the keyboard mechanism of the reproducing piano.[4] A third electromagnetic pen is connected in circuit with a contact key which records measure or phrase duration. The pen which records pedal depression is connected with the kymograph in such a way that depression of the pedal activates the pen downward, while release of the pedal activates the pen upward.

Utilizing the kymographic method of recording pedal performance, a rendition of Schumann's "Traumerei," Op. 15, no. 7, by Ethel Leginska (Duo-Art roll no. 55860) was compared with a rendition of the same composition by Leopold Godowsky (Duo-Art roll no. 5855-3). The tempo of each rendition was regulated in

[3]It should be noted that the reproducing mechanism of the Duo-Art piano does not provide half-depression or half-release. In other words, the many variations and gradations of pedal amplitude which prevail in a human performance are not reproduced by this instrument. The pedal bellows function in whole and not in part. If a half-depression appears on the kymogram, it is due to the fact that the pedal bellows are somewhat sluggish in responding to a very short pedal perforation on the recording roll. This means that many nuances are introduced in a rendition at points where the artist probably had no intention of introducing them. Despite this artificial condition, the Duo-Art piano is quite satisfactory for the purpose of many comparative studies of the frequency and protensity of damper-pedal depression.

[4]For the method of recording the incidence of melodic pitch points on a kymographic drum, see (3).

TABLE 1

A Direct Comparison of the Damper-Pedal Performances of Ethel Leginska and Leopold Godowsky Playing Rubinstein's Arrangement of Schumann's "Traumerei"

In treating the successive interpretations statistically, the composition is divided into eight phrases of four measures each. The time relationships and the pedal frequency have been computed from kymograms recorded by the Duo-Art reproducing piano.

Rendition by Ethel Leginska			Rendition by Leopold Godowsky		
Phrase	Time in seconds	Pedal frequency	Phrase	Time in seconds	Pedal frequency
1	17.8	6	1	21.6	17
2	19.3	7	2	21	16
3	17.5	6	3	19.5	17
4	20	7	4	21.1	19
5	17.2	6	5	15.8	18
6	18.8	7	6	20	15
7	17.7	6	7	19.6	16
8	28.3	6	8	31	17
		51 Total			135 Total

conformity with the roll indications. The intensity lever was shifted to Duo-Art "normal" reading.

For convenience of presentation, the results of each rendition are given statistically in Table 1. In this simple statistical treatment, the composition is divided into 8 phrases of 4 measures each. The time allowed to play each phrase is computed in tenths of a second as the smallest time unit from the first pitch point of one phrase to the first pitch point of a subsequent phrase and is derived from the original kymogram through a micrometric study of the time line by means of a powerful reading lens.

Even a casual glance at the figures represented in this table will convince any reader of the vast difference in the pedal performances of these two outstanding artists. Leginska pedals 51 times during the rendition, while Godowsky pedals 135 times, or nearly three times as many depressions. Leginska allows 2 minutes and 36.6 seconds in which to play the composition, while Godowsky allows 2 minutes and 49.6 seconds. For any given phrase, Leginska never employs the same pedal pattern as Godowsky, nor does she ever allow the same time value. Leginska's pedalling is essentially harmonic in structure, while that of Godowsky is primarily melodic in structure. Leginska's tendency is to shift when there is a basic

change of harmony, while Godowsky's tendency is to pedal passing notes either in the treble or in the bass. Both pianists frequently indulge in syncopated pedalling, i.e., pedalling marked by either anticipation or procrastination and not coincident with the pitch points involved. No attempt is made in this study to relate pedal performance with other musical attributes, such as tonal intensity of either the treble or the bass pitch region and the temporal value of phrase formulation. There is little doubt that these factors, in addition to many other keyboard conditions, do play a major rôle in fashioning the pattern of any pedal performance. However, the author's chief concern at this time is merely to demonstrate the simple, undeniable fact that considerably wide differences do exist between the pedal performances of great artists.

While the figures in Table 1 serve to illustrate some basic differences in performance, the true characteristics of each rendition are revealed only in a study of the pedal patterns as recorded on the

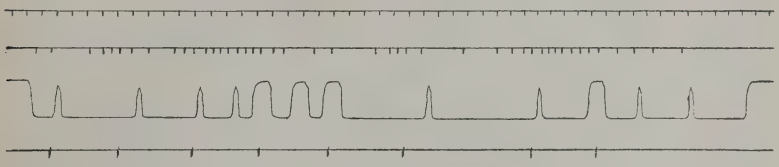

FIGURE 1
LEGINSKA'S RENDITION

original kymograms. Figure 1 is a reduced photostatic reproduction of a kymogram giving Leginska's damper-pedal performance for the last two phrases of Schumann's "Traumerei." The top line is the time line, the line immediately below is the melodic line, which gives the pitch point incidence of the melodic contour; the wave pattern below the melodic line is the pedal line, while the line in lowest position gives the division of musical measures within the phrases.

Figure 2 gives Godowsky's rendition for the same two musical phrases. In a brief comparison of these two renditions, in addition to the wide differences in pedal pattern, the reader will notice some marked differences in the temporal relationships of the pitch points of the two melodic lines. For example, in the last two measures, Leginska, playing somewhat more rapidly, groups her melodic points together in quite a different fashion. While figures are satisfactory

in indicating pedal depression frequency, the protensity characteristics of the pedal pattern are likewise revealed only through a direct analysis of the kymogram. The remark that the damper-pedal was depressed four times within a phrase, although convenient and satisfactory in suggesting the broader aspects of response, does not indicate minute variations in performance, nor does it reveal the actual distribution of the depressions in relation to the melodic contour. Hence, for purposes of illustrating the true variations in pedal per-

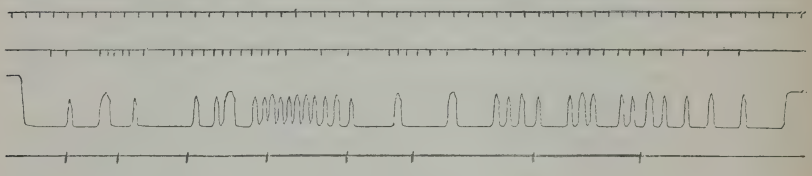

FIGURE 2
GODOWSKY'S RENDITION

formance, Figures 1 and 2, as well as the several subsequent excerpts from the original kymograms, are inserted.

The most naïve listener can readily differentiate between Leginska's performance and that of Godowsky. The performance of Leginska is often bathed in a harmonic background, rich in overtones, whereas the performance of Godowsky, despite greater keyboard sustension, is more sharply outlined and melodically projected from a soft harmonic background unsupported by open strings through frequent release of the pedal. Despite these marked differences in pedal technique, which lead to entirely different musical effects, any thoughtful critic will hesitate to exclaim that either of these renditions is the "correct" one. Since master virtuosos can render such diverse interpretations of a relatively simple composition, and since, after all, musical interpretation is engendered by individual taste and reflects in some measure the emotional characteristics and eccentricities of the performer, any reference to a condition of "correctness" is largely irrelevant. Both of these renditions under consideration are "correct" in so far as neither violates the fundamental principles of pedal decency. From the standpoint of musical merit, either rendition is as "good" as the other.

Yet, admitting the fact that these two entirely different pedal formulations are based upon wholly acceptable technical criteria,

few auditors, indeed, will pass equal judgment on these renditions. From the hedonistic point of view, this condition is to be expected. Levels of appraisal are inevitable from any group of individuals consisting of various perceptual types of listeners. The whole subject of musical appreciation which revolves about this point is so distressingly complex that space limits any further discussion of this aspect.[5]

It is generally held that, because of preciseness of form in phrasal repetition, certain primitive rhythmic patterns are subject to a minimum of variation in interpretation. Such primitive rhythmic patterns prevail in music written in simple march time. Beethoven's "Turkish March," for example, which is a composition characterized by a simple melodic figure superposed upon a simple, conventional harmonic background productive of regular recurring accents, falls within this category. In any reduction of the relatively complex to the relatively simple, one might be easily led to suppose that in musical composition which is characterized by primitive patterns in all attributes, the probability for any marked differences in pedal performance is extremely small. It would seem logical to maintain that pedal differences ought to diminish in scope when a specific formulation of notes tends to delimit the number of interpretations possible. This does not seem to be entirely the case, however.

In comparing Harold Bauer's rendition of Rubinstein's arrangement of Beethoven's "Turkish March" (Duo-Art roll no. 5837-4) with Joseph Hofmann's rendition of the same composition (Duo-Art roll no. 6327-8), one is surprised to learn the marked differences which exist in the pedal performances of these two famous artists. In effecting this comparison statistically, the composition is divided into 24 phrases of four measures each, and four phrases of two measures each. The results obtained from the kymographic method of recording are presented in Table 2. Bauer pedals 99 times during the rendition, while Hofmann pedals 89 times. The pedalling of both pianists is primarily of the rhythmic type, i.e., the pedal is usually depressed on the fundamental measure accent. Bauer allows 3 minutes and 57 seconds in which to play the composition, while Hofmann allows 3 minutes and 51.8 seconds—a remarkably close approximation. In view of the exact rhythmic formulation of the composition, such similarity in time value is to be expected. From the standpoint of distribution, however, no two parallel phrases of

[5]For information regarding the nature of individual differences in listening to music, see (4).

TABLE 2

A DIRECT COMPARISON OF THE DAMPER-PEDAL PERFORMANCES OF HAROLD BAUER AND JOSEPH HOFMANN PLAYING BEETHOVEN'S "TURKISH MARCH"

In treating the successive interpretations statistically, the composition is divided into 24 phrases of four measures each, and four phrases of two measures each. The two-measure divisions are indicated by asterisks following the phrase numbers. The time relationships and the pedal frequency have been computed from kymograms recorded by the Duo-Art reproducing piano. The time value of the last phrase of the composition is calculated from the tonal incidence of the initial note of the first measure to the point of final pedal release in the last measure.

Rendition by Harold Bauer			Rendition by Joseph Hofmann		
Phrase	Time in seconds	Pedal frequency	Phrase	Time in seconds	Pedal frequency
1	9.3	4	1	9.0	1
2	9.2	2	2	8.8	2
3	9.1	5	3	8.8	0
4	8.8	3	4	8.4	4
5	9.1	3	5	8.6	2
6	8.8	4	6	8.5	6
7	8.9	4	7	8.7	4
8	9.0	4	8	8.5	5
9*	4.6	2	9*	4.5	3
10	9.0	4	10	8.4	5
11*	4.5	2	11*	4.6	4
12	9.1	3	12	8.9	3
13	9.2	3	13	8.4	3
14	9.3	4	14	8.7	3
15	9.1	4	15	8.4	1
16	9.0	4	16	8.8	4
17*	4.7	2	17*	4.4	4
18	9.3	4	18	8.7	4
19*	5.0	2	19*	4.7	2
20	9.4	4	20	9.6	2
21	9.6	4	21	8.8	1
22	9.5	4	22	9.1	7
23	9.5	4	23	8.8	3
24	9.5	4	24	8.8	6
25	9.1	3	25	9.0	3
26	9.0	4	26	8.9	0
27	8.8	6	27	9.3	4
28	7.6	3	28	11.7	3
		99 Total			89 Total

either rendition are allowed the same time values. The chief time difference for any two parallel phrases occurs at the close of the composition. Bauer allows 7.6 seconds for the last phrase, while Hofmann allows 11.7 seconds for the same phrase. In regard to a few outstanding differences in pedal performance, in only nine phrases

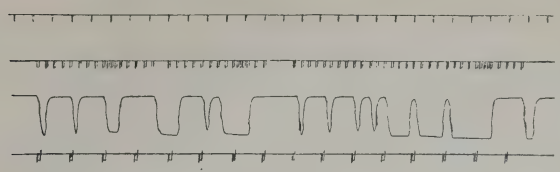

FIGURE 3

HAROLD BAUER'S PEDAL PERFORMANCE OF THE OPENING PHRASES OF THE "TURKISH MARCH"

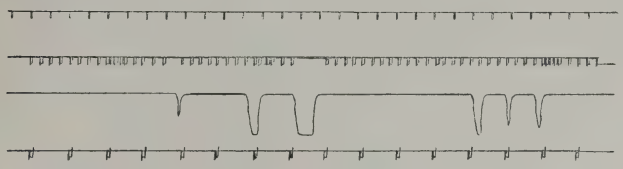

FIGURE 4

JOSEPH HOFMANN'S PEDAL PERFORMANCE OF THE OPENING PHRASES OF THE "TURKISH MARCH"

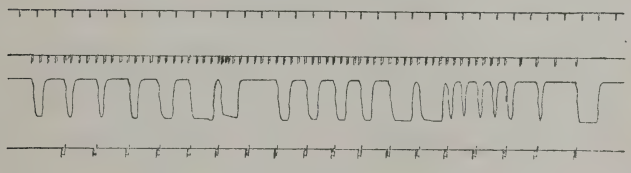

FIGURE 5

HAROLD BAUER'S PEDAL PERFORMANCE OF THE CLOSING PHRASES OF THE "TURKISH MARCH"

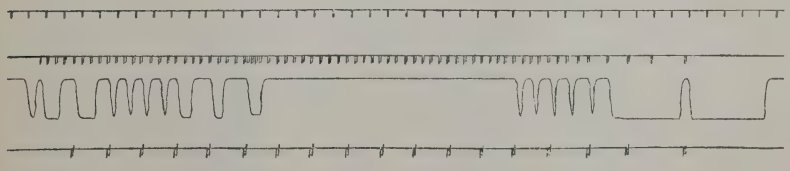

FIGURE 6

JOSEPH HOFMANN'S PEDAL PERFORMANCE OF THE CLOSING PHRASES OF THE "TURKISH MARCH"

TABLE 3

A Comparison of the Damper-Pedal Frequency between a Pianoforte Performance of Schumann's "Traumerei" and its Repetition Following a 10-Minute Rest Interval

The results obtained from four trained musicians are presented in tabular form. The composition is statistically divided into eight phrases of four measures each. An exponent above a number refers to half pedal releases: a sub-numeral refers to half pedal depression. Compare the results below with those given in Table 1.

Phrase number	First rendition Time in seconds	First rendition Pedal frequency	Second rendition Time in seconds	Second rendition Pedal frequency
		Subject I		
1	18.5	10_2	17.2	10_3
2	19.7	13	19.3	12
3	18.3	11_2	17.3	12_1
4	19.6	12	19.4	12
5	18.5	14	18.5	13
6	18.2	14	19.1	13_1
7	17	11_1	17	8_5
8	32.4	15	31	14
		100_5 Total		94_{10} Total
		Subject II		
1	19.2	11^1	22.2	10^1
2	22.8	10^1	26.2	10^1
3	20	12^4	23.5	7
4	23	13^1	26.7	8^1
5	16.4	11^4	20	7^2
6	19	12^3	23	8
7	18.3	9	21	8^1
8	26.5	10^2	30.9	9
		88^{16} Total		67^6 Total
		Subject III		
1	19.8	10	20	9
2	20	11	21	14
3	19.5	11	19.3	11
4	19.2	11	21.5	13
5	18.3	12^1	18.4	14
6	20.2	12	20.5	11
7	19.8	10	20.5	12
8	28	13	27.5	15
		90^1 Total		99 Total

	Subject IV			
1	20.4	11_2	19.4	10_1
2	19.1	13	21	13
3	20	12	19.2	14^1
4	19.3	14	20.6	13
5	17.7	15	18	13_3
6	19.8	13	18.5	15
7	17.4	12_1	18.2	11
8	29.5	14	29.8	13
		104_3 Total		102^1_4 Total

out of a total of 28 phrases does pedal frequency for the two renditions coincide. And even in these few coincidences, the distribution of pedal pattern in relation to the pitch points involved is quite different. In phrase 3, Bauer pedals five times, while Hofmann does not pedal at all. In phrase 15, Bauer pedals four times, while Hofmann pedals once. In phrase 22, Hofmann pedals seven times, while Bauer pedals only four times. And in phrase 26, Bauer pedals four times, while Hofmann does not give any response. Figures 3 to 6 inclusive are photostatic reductions of the original kymograms, which assist in illustrating at a glance some of the marked differences in pedal performance. Figure 3 gives Bauer's pedal performance for the introductory phrases of the "Turkish March," while Figure 4 gives Hofmann's pedal performance for the very same musical section. Figure 5 shows Bauer's performance for the closing phrases of the composition, while Figure 6 gives Hofmann's performance for the same sequence of measures. In these respective excerpts, the top line is the time line in seconds, the line immediately below is the melodic line, the wave pattern is the pedal performance, while the bottom line indicates the measure span.

Four trained pianists were employed in an experiment which was planned to demonstrate the existence of individual differences in pedal performance and also the extent to which a musician is capable of duplicating voluntarily a pedal response to a given musical composition. Again, Schumann's "Traumerei" was chosen for this purpose. The subjects were carefully examined for their musical ability and were selected from a large group of candidates. Subjects 1 and 2 are conservatory graduates and at the time of experimentation were occupied in professional work. Subjects 3 and 4 have both had the equivalent of four years of preparatory training.

Before selection was effected, in addition to the questionnaire method of obtaining information, each subject was required to prove his ability to perform from memory and to read unfamiliar compositions of moderate difficulty at sight. The compositions chosen for this purpose were those written by Nyiregyhazi, namely, "Theme in A minor," "Fantasy in D minor," "Plaintive Sounds," and "Adagio from a Dramatic Sonata in A-flat minor." These compositions were chosen especially for their unusual modulations and frequency of accidentals introduced through change of key.

As a preliminary task in experimentation, each subject was required to play through "Traumerei" in order to convince the director that he could perform this composition without any significant error. Upon satisfying this requirement, the subject was requested to play the composition a second time, this time, however, observing a definite tempo value. This tempo value was established by presenting to each performer before rendition a series of 16 metronome clicks. An eighth note was evaluated at a metronome indication of 100. In the process of establishing this time value, each subject was instructed to orient himself by a mental count of each metronome beat. Upon cessation of the metronome pendulum at the 16th beat, the performer was then instructed to begin his rendition, and to approximate as a time basis the metronome equivalent. The performer was made to understand, however, that the metronome beat was preliminarily introduced to serve merely as a guide to a desirable tempo, and that his own interpretation was not to be mechanized nor delimited by this specific temporal span. Neither in the preliminary nor in the final instructions was any reference made to pedalling. The subject was simply told to give an artistic rendition of the composition according to his own method of interpretation. No pedal symbols were included in the score from which the subject read. By means of special apparatus concealed from view, the subject automatically recorded his own pedal performance.

Figure 7 is a diagrammatic sketch of the method of recording the pedal performance of the several musicians tested. The pedal rod *R*, which is supported by the pedal leg *L*, activates the damper mechanism within the piano through the depression of the pedal *A*. This pedal rod is connected in series with an inverted N-pulley system, which is capable of transferring to a kymographic drum the amplitude and protensity characteristics of any series of pedal depressions. The pedal pattern is amplified by means of the intermedi-

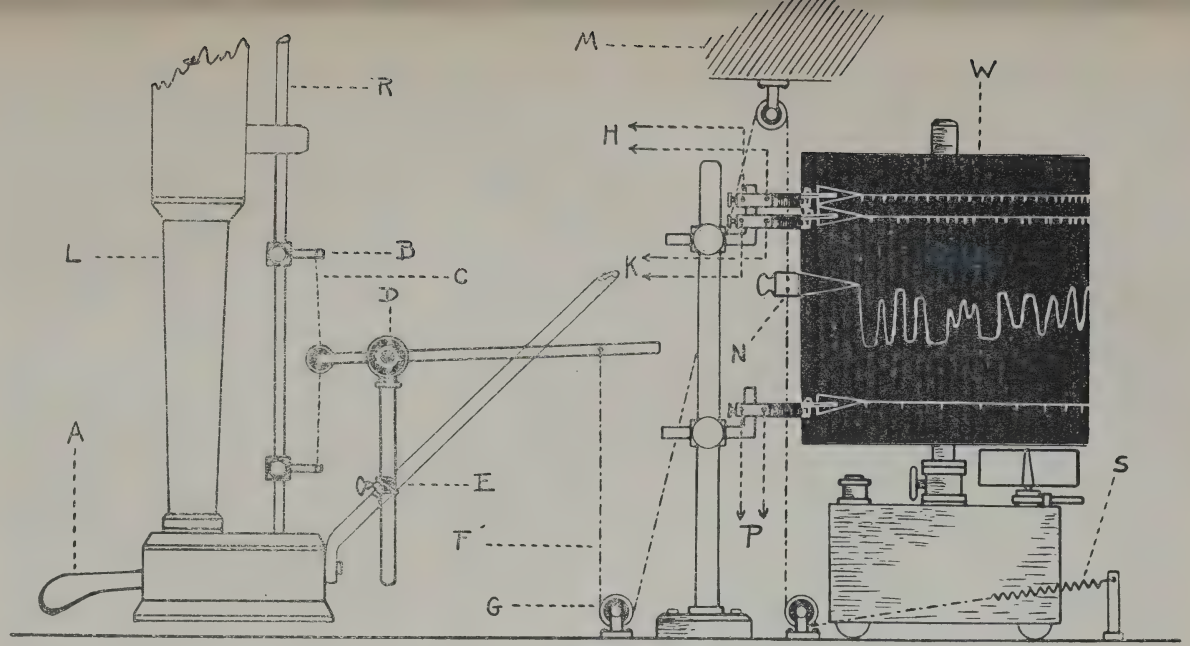

FIGURE 7

MECHANISM FOR RECORDING PIANO PEDAL PERFORMANCE

A—damper pedal; *B*—clamp to which is attached heavy silk thread *C*; *D*—fulcrum adjustment of lever device; *E*—clamp attachment to piano support; *F*—heavy silk thread passing through a series of three minute brass pulleys and drawn tense by the spring *S*; *G*—typical brass pulley; *H*—circuit line from seconds pendulum to first electromagnetic pen; *K*—circuit line from melody key to second electromagnetic pen; *P*—circuit line from phrase key to third electromagnetic pen; *M*—piano base acting as support for superior pulley; *N*—damper-pedal recording pen constructed of thin aluminum. Alignment of this pen with the drum is maintained by a twisted winding of the thread. *W*—kymograph drum.

ate lever device *D*, whose fulcrum is guaged at an arbitrary mechanical advantage of three points. Following the use of the reversible pulley system, any depression of the pedal equivalent to an *R* extensity will depress the recording pen *N* for an amplitude equivalent to an extensity of 3*R*. Total release of the damper pedal draws the recording pen upward to zero position. Three electromagnetic pens, whose circuit lines are indicated at H, K, and P, are employed to record (1) the time of performance in seconds, (2) the pitch points of the melodic series, and (3) measure of phrase incidence. As in previous instances, these several functions are recorded electrically, and through this recording process it is possible to obtain a complete picture for purposes of statistical analysis.

After the subject had completed his first rendition of the composition (the preliminary rendition is no longer considered in this discussion), he was dismissed from the music room for a period of ten minutes. During this interval his time was chiefly occupied with reading in the departmental library. This dismissal also gave the experimenter ample time and opportunity to effect the necessary kymographic adjustments for a second record. At the conclusion of the rest interval, the subject was recalled and instructed to give an exact duplicate of his first rendition in every detail. On this second occasion, no preliminary metronome value was indicated. The duty of the subject was to recall his previously established tempo as well as all of the other factors of interpretation. During both renditions, the subject was permitted to read from a score without pedal symbols.

The results obtained from this simple experiment are strikingly significant. *No subject was able to duplicate his previous performance.* On the whole, the professional pianists evidenced greater variation in the duplicate response than did the student pianists. From the introspective reports obtained from the subjects immediately following the second rendition, subjects 2, 3, and 4 declared that they were certain of having achieved an identical repetition. Subject 1 stated that he did not believe it humanly possible to duplicate any given musical performance of appreciable length. The results in terms of pedal frequency are presented in Table 3. In reading this table, which is presented for statistical convenience, the reader should not be misled by the identity of pedal frequency in the case of several parallel phrases. An analysis of the original kymograms shows that no subject had achieved an exact repetition of his initial

pedal performance for any given phrase. In every instance, the pattern distribution for any two parallel phrases was different. In regard to the total time allowed to play the composition, no subject was able to duplicate his first rendition. While subject 4 closely approximated in repetition the time value of his initial performance, it must be remembered that his total time summates from non-coincident phrases.

Following the precise beat of the metronome, the progression of notes constituting the entire composition possesses a time value equivalent to approximately 2 minutes and 32 seconds. The time allowed by each of the four subjects for each rendition of the composition is as follows:

Subject	First rendition		Second rendition	
	Minutes	Seconds	Minutes	Seconds
1	2	42.2	2	38.8
2	2	45.2	3	13.5
3	2	44.8	2	48.7
4	2	43.2	2	44.7

Subject 2 shows the greatest amount of variation in performance repetition, both in regard to tempo and in regard to pedal depression. From the standpoint of musical merit, however, both renditions proved equally good. In the first rendition, his pedalling assumed the melodic type, whereas in the second rendition, his tendency was to prolong a specific harmonic background. Differences in the time values of phrasing and differences in intensity shadings were instrumental in effecting these pedal changes. In the first rendition, he made a total of 88 depressions, while, in the second rendition, he made a total of only 67 depressions. In the first rendition, he released the pedal halfway 16 times, while, in the second rendition, he made only 6 such movements.

Subject 1 frequently indulged in half-depressions. In the second rendition, he made twice as many half-depressions as he did in the first rendition. It must be noted that the musical effects produced by half-release and by half-depression are quite different from those produced by complete release or complete depression. Half-depression or half-release gives the harmonic background a blanketed and subdued effect. While resonance still functions, it is diminished to a considerable extent. In contrast to both subjects 1 and 2, subject

3 made a single half-release in the first rendition and no half-depression in either rendition.

The pedalling of subject 4 was essentially melodic. This subject was able to approximate more closely than any other subject the time value as well as the pedal frequency and distribution of his initial rendition. It is singular to note, however, that either of his renditions was the least artistic of the group. His performances, on the whole, were quite stilted and admitted of very few shadings in intensity. Although his tempo was slightly in advance of the metronome basis, his phrasing throughout was unusually rigid and mechanized in conception.

Summary

1) Teachers of the pianoforte frequently avoid teaching pupils the principles of damper-pedalling. In most instances, the pupils are allowed to invent their own rules of pedalling. Such individual inventive schemes often lead to disastrous results in performance.

2) Probably one of the most serious reasons for the neglect to define more accurately where the damper-pedal is to be used is that the greatest virtuosos do not agree among themselves as to where or where not a particular pedal pattern should be introduced.

3) Employing the kymographic method of comparison, it has been shown that leading artists differ considerably in their pedal interpretations of a given musical composition.

4) Relative to intensity shadings, speed of rendition, and types of phrasing effected, it has been shown that several different damper-pedal patterns may be technically acceptable and musically appropriate for a given composition.

5) Four well-trained pianists, in addition to evidencing marked individual differences in interpretation, were unable to duplicate after a ten-minute interval an initial pedal performance.

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UNE DISCUSSION DE LA NATURE DE L'USAGE DE LA PÉDALE FORTE DU PIANO AVEC UNE ÉTUDE EXPÉRIMENTALE DE QUELQUES DIFFÉRENCES INDIVIDUELLES DANS L'USAGE DE LA PÉDALE

(Résumé)

On a fait une enquête expérimentale dans le but de constater la nature, la fonction, et le degré de variabilité dans l'usage de la pédale forte du piano par des pianistes fameux. Le piano méchanique Duo-Art a reproduit quelques compositions comme jouées par quatre pianistes célèbres pour rendre possible l'analyse de leur interprétation de ces compositions bien connues. On a noté l'usage individuel de la pédale au moyen d'un kymographion continu de papier au moyen d'une connexion directe avec le méchanisme de la pédale et synchronisé avec la notation musicale au moyen d'une machine électrique spéciale. Les résultats montrent de grandes différences dans l'usage de la pédale par les divers artistes.

On a employé quatre pianistes habiles, deux étudiants, et deux pianistes de métier, pour montrer l'existence de différences individuelles dans l'usage de la pédale et aussi le nombre de fois qu'un musicien est capable de répéter volontairement un usage de la pédale dans une composition donnée. Les résultats de cette expérience montrent que nul pianiste n'a pu répéter son premier usage de la pédale. Dans tous les cas on s'est servi de la pédale d'une façon différente pour deux phrases parallèles. Ces différences sont attribuées aux différences dans le tempo des phrases et aux différences dans les nuances de l'intensité. On conclut que par rapport aux nuances de l'intensité, la vitesse de jouer, et les types de phrases infuencées, plusieurs usages différents de la pédale peuvent être techniquement acceptables et musicalement convenables pour une composition musicale donnée.

HEINLEIN

EINE ERÖRTERUNG DER ART DES GEBRAUCHS DES DÄMPFER-PEDALS AM KLAVIER, NEBST EINER EXPERIMENTELLEN STUDIE EINIGER INDIVIDUELLER UNTERSCHIEDE IN DER ANWENDUNG DES PEDALS

(Referat)

Man unternahm diese experimentelle Untersuchung um die Art, die Funktion und den Grad der Variabilität in dem Gebrauch des Pedals berühmter Klavierspieler zu bestimmen. Duo-Art Klavierplatten der Vorträge vier

hervorragender Klavierspieler wurden in der Analyse der Interpretationen bekannter Kompositionen angewandt. Individuelle Muster des Pedal gebrauchs wurden mit einem continuirlichen Papier Kymographen durch direkte Verbindung mit dem Pedalmechanismus des Klaviers aufgenommen und wiederum synchronisiert mit den Noten der Komposition vermittelst einer besondern elektrischen Vorrichtung. Die Ergebnisse erweisen grosse Unterschiede im Pedalgebrauch der verschiedenen Künstler.

Vier geübte Klavierspieler, zwei Studenten, und zwei Künstler von Beruf, sollten die individuellen Unterschiede im Pedalgebrauch demonstrieren und zugleich dartun in wiefern ein Spieler bei einem Vortrag den Pedalgebrauch nach Wilkür genau wiederholen kann. Die Ergebnisse des Experiments bewiesen dass keine Vpn. den ersten Vortrag genau wiederholen konnte. In allen Fällen war das Verteilungsmuster für irgend welche parallelen Tonsätze verschieden. Diese Unterschiede des Pedalgebrauchs werden wohl durch Unterschiede der Zeitdauer, der Tonsätze und der verschiedenen Abtönung der Intensität bedingt. Man kam zu dem Schluss, dass in Beziehung auf die Abtönungen der Intensität, die Schnelligkeit des Spiels, und die Art der melodischen Gruppirung des Tonsatzes, mehrere verschiedene Pedalmuster für eine gewisse Komposition technisch annehmbar, und musikalisch passend sein können.

HEINLEIN

ПРИРОДА УПОТРЕБЛЕНИЯ ЗАГЛУШАЮЩЕЙ ПЕДАЛИ И ЭКСПЕРИМЕНТАЛЬНОЕ ИЗУЧЕНИЕ НЕКОТОРЫХ ИНДИВИДУАЛЬНЫХ РАЗЛИЧИЙ В УПОТРЕБЛЕНИИ ПЕДАЛИ.

(Реферат).

Было проведено экспериментальное исследование природы, функций и степени изменчивости употребления заглушающей педали у знаменитых пианистов. Были употреблены записи исполнения известных произведений четырьмя знаменитыми пианистами; записи воспроизводили на репродуцирующем пианино Duo-Art. Индивидуальное употребление педали каждого пианиста было записано на непрерывном кимографе, непосредственно соединенном с механизмом левой педали, и синхронизировано с музыкальными обозначеннями путем специальной электрической установки. Результаты показали резкие различия в употреблении педали разными музыкантами.

На примере четырех тренированных пианистов (двух студентов и двух профессионалов) было демонстрировано существование индивидуальных различий в употреблении педали, а также было показано, до какой степени музыкант способен произвольно повторить пользование педалью при вторичном воспроизведении данного музыкального отрывка. Оказалось, что ни один из испытуемых не мог в точности воспроизвести свое первоначальное исполнение. В каждом случае распределение педального рисунка было иное. Эти различия можно приписать разнице во временном распределении фразировки и в интенсивности оттенков. Был сделан вывод, что в зависимости от употребленной интенсивности оттенков, быстроты исполнения и типа фразировки для каждого музыкального произведения можно принять несколько различных образцов употребления левой педали.

Гайнлайн (Heinlein).

SHORT ARTICLES AND NOTES

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APPARATUS FOR SMOKING KYMOGRAPH DRUM PAPERS

A considerable technical difficulty which most students, and at times some investigators, have is that of smoking "drums" satisfactorily for record purposes. The best surface is one on which the soot is evenly deposited and is dark enough to make the finished record readily legible. With the ordinary arrangements for smoking, however, the soot is often laid on in blotches, and to make the surface look smooth there is the tendency to go over it a number of times, using more and more smoke until there is produced a uniform black surface. That surface, having a relatively thick carbon deposit, does not offer the best condition for many purposes, because the soot tends to collect upon the writing points and the lines become heavier as the experiment proceeds. Moreover, the user of the kymograph, especially the beginner, wastes much paper in his efforts to get a smooth, not too dark, and not too light surface.

The apparatus, of which working drawings are here shown, enables the beginner to smoke drums corresponding in quality to those of the expert; it saves paper, and it gives the student or the investigator much time which is now dissipated in the acquisition and application of a technique for the main business of his experiment.

The essential parts of the apparatus are the paper covered drum which is to be smoked, the burner, and the mobile relations of these to one another. Figure 1 shows the apparatus in front elevation, and Figure 5 shows it in cross-section (almost end elevation). Figures 2, 3, and 4 show details of the mechanism, so that a good mechanic will be able to manufacture a duplicate. The part which is peculiar, in the sense of being unique and making possible its operation, is the endless or double-pitched screw which moves the smoking burner to and fro.

The general mechanism and operation of the apparatus is as follows (refer to Figure 1): The drum *A* covered with glazed paper is placed so that the lower part of its spindle is held by the clutch *B*. The burner *H*,

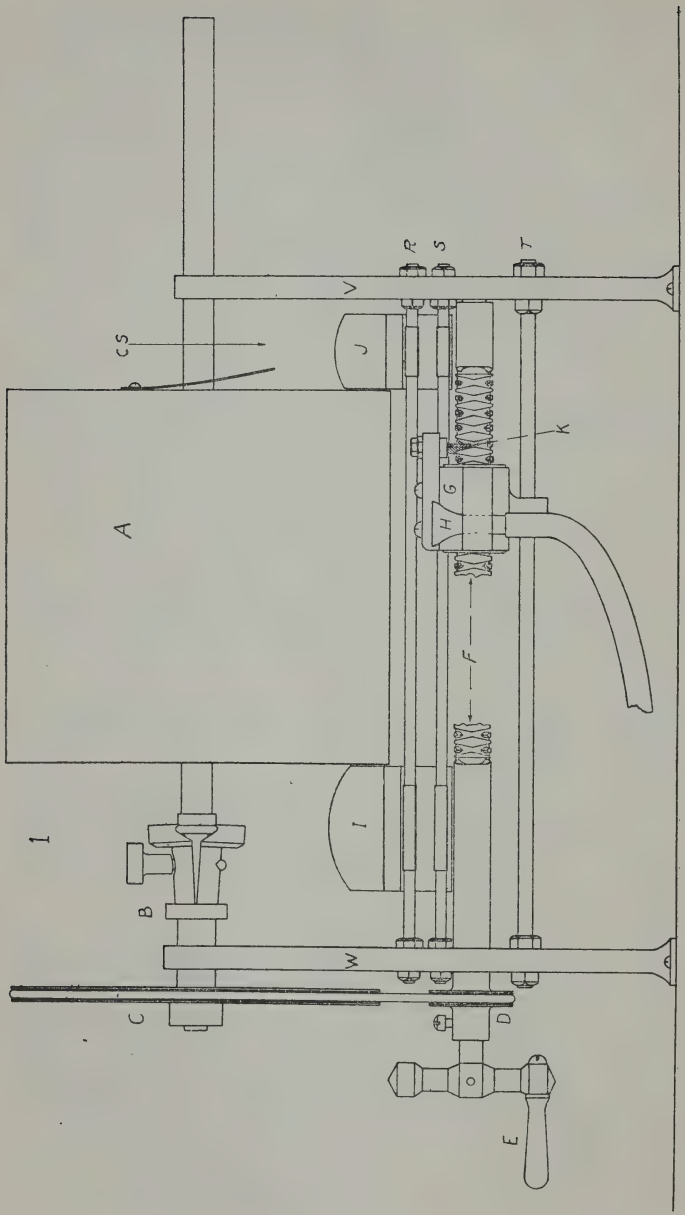
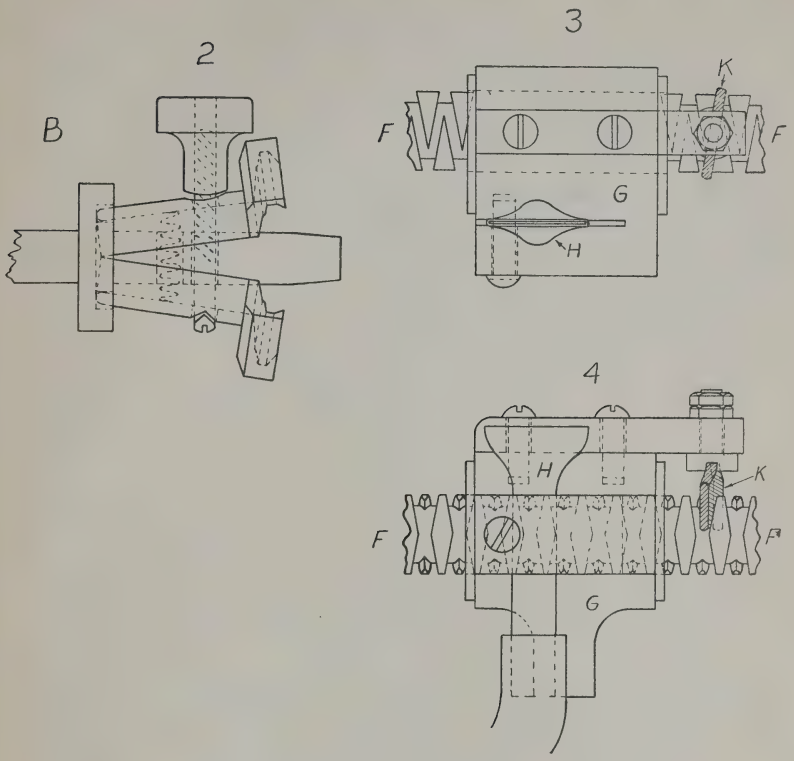


FIGURE 1

FRONT ELEVATION OF DRUM-SMOKING APPARATUS WITH KYMOGRAPH DRUM IN POSITION



FIGURES 2, 3, AND 4

FIGURE 2—DETAILS OF CLUTCH; FIGURE 3—DETAIL PLAN OF DOUBLE SCREW *F* WITH FINGER *K* AND BURNER *H*; FIGURE 4—DETAIL ELEVATION OF DOUBLE SCREW *F* AND CARRIAGE

which is under one of the paper protectors (*I* or *J*) is lighted, and the handle *E* is turned at a regular speed, about once in two seconds, so that the endless or double screw *F* and the drum *A* move simultaneously, the latter at a slower speed than the former. As the screw revolves, the burner carriage moves forward until the finger *K* arrives at the end. At that point the finger automatically turns and causes the carriage to go in the opposite direction, until the finger again reaches the other end of the screw, while the drum continues to revolve in the original direction. The to-and-fro movement of the carriage (and its burner) continues until the glazed paper is smoked to a sufficient degree of darkness, and it is stopped when the burner is under one of the protectors (*I* or *J*).

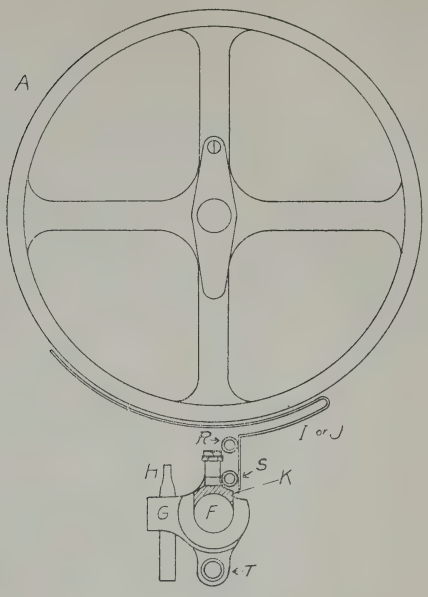


FIGURE 5

SECTION THROUGH DRUM-SMOKING APPARATUS AT POINT MARKED *CS* ON FIGURE 1

For purposes of manufacture, reference should be made especially to the detail drawings. In Figure 1, three items remain to be explained, *R, S,* and *T*. *R* and *S* are tie rods between the uprights, *V* and *W*, which support the drum and the revolving screw. These tie rods also permit a horizontal adjustment of the protectors, *I* and *J*, so that the ends of the papers covering the kymograph drum shall not be burned. This adjustment of the protectors obviates the necessity of too accurate placement of the drum on its shaft in relation to the upright supports *V* and *W*. The third horizontal rod *T* helps to keep the burner carriage firm. The positions of these rods in relation to the other parts are shown in Figure 5.

Figure 2 shows details of the "clutch." This has been constructed primarily for use in connection with the Harvard kymograph. The hollow shaft of the drum fits over the spindle and the base of the shaft is held tightly by screwing the large thumb nut which is indicated. A spring (also shown) is used so that when the clutch is unscrewed it will open widely to permit the ready release of the drum shaft. The clutch is easily changed to fit the spindle of any other kymograph drum, e.g., the Zimmermann.

Different views of the double-cut-thread shaft *F* are shown in Figures 3

and 4. The relations to it of the carriage *G* and the movable finger *K* are also shown in the same drawings. The fish-tail burner *H* is also shown. The gas supplied to this institution gives a smokeless flame with the minimum of air, and because of this the gas is sent through a wash bottle containing benzene or benzol before it reaches the burner. This gives an excellent, even smoke when the gas is regulated at the inlet.

The whole apparatus is placed on a wooden base. As now in use the smoker has been changed in only one particular from the indicated drawings, viz., the two pulleys (*C* and *D*) have been replaced by sprocket wheels (44 and 18 teeth, respectively), and the pulley by an endless chain.

The double-thread screw is made of steel, the movable finger is of phosphor bronze, and the remainder of the apparatus is brass.

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APPARATUS FOR PERCEPTION OF FORCE OF MOVEMENT

For the consideration of kinaesthesis, numerous experiments are readily made on the extent of movement. More difficult to perform and less satisfactory means for performance are the experiments on the force and on the time of movement. In the work of Fullerton and Cattell (1) an apparatus of the nature of a spring balance was used for determining the perception of force. In their instrument the force was a constant function of the extent, as is found in the common spring balance. Estimation of the accuracy of perception of force in using such an instrument can be made only by comparison and interpolation of the extent-perception factor. An apparatus in which the extent-constant can be varied makes the student observer pay special attention to the factor of force, although he may not entirely disregard the perception of extent. The instrument here described has been used in the elementary course in experimental psychology, and because of the possibility of its variation in different experiments and its ease of adjustment it has been found satisfactory.

Figure 1 shows the plan and end elevation of the apparatus. *A* is the base, made of maple, on which the elements are assembled. The main elements are the extension spring *B* and the pull handle *D*. The spring is held rigid at one end on a metal angle by a screw *G*, which may be moved to and fro and made fast at any special point by the set nut. At the other end of the screw is the swivel hook to which the spring is immediately attached. To the other end of the spring is attached a lever *C*, upon which the pull is made by the subject through the handle *D*. The pull has a round handle, and it is attached to the lever by a snap. Upon

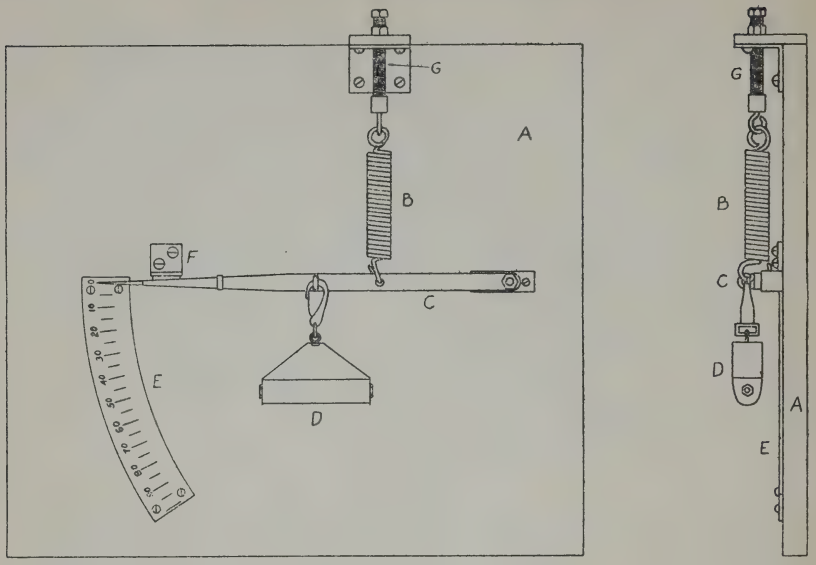

FIGURE 1

PLAN AND END ELEVATION OF FORCE OF MOVEMENT APPARATUS

the lever are two eyes, to either one of which the handle may be attached, thus changing the extent-constant for force in relation to the leverage employed. The lever is stopped by the angle stop *F*, on the face of which is a layer of soft rubber. The force of the movement (and its extent) is read from the scale *E*. The divisions are arbitrary, because the spring tension or the leverage may be changed. By turning the screw *G*, the initial tension of the spring, before the lever moves, may be changed from zero to two or more kilograms. This adjustment is made readily, and the subject may be called upon to make comparison of the force of one movement which starts in extent at zero with another movement which starts at three kilograms. Thus, the subject may be required to make an initial pull extending the spring from the beginning of the movement until he reaches the point 50 on the scale, the equivalent of about five kilograms. Then the screw *G* is set so that the spring is initially extended to three kilograms, which must be pulled before the lever begins to move, and the completion of the pull to 20 would make the two movements of the same force. Or, leaving the screw so that the spring is extended only when the pull begins, one trial may be made by attachment of the handle to one eye on the lever and the second comparison or trial when the handle is attached to the second eye. The eyes are in relation to the lever pivot

as 2:3, and in relation to the spring suspension they are 1:2.5. If the student is to be saved the trouble of calculation, a table may be constructed for definite settings of the apparatus.

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A RECORDING PENCIL MAZE

As a class experiment in learning, the pencil maze presents a relatively complex problem, which can be mastered individually in a few hours and which has considerable collective variability. When, however, the movements must be recorded by an observer, while at the same time the observer may be busy recording whatever introspections or remarks are made by the subject, much may be lost. The apparatus shown in the accompanying drawings is a maze which in general in an extension of one originally used by Boring in his study of learning by the insane. It is constructed so that the subject (blindfolded) traces his pathway onto the paper, which is a permanent record available for the calculation of numbers of errors, or error repetitions, etc. It is also constructed in such fashion that four maze patterns are available for the same direction of the subject's movements from the starting-point to the goal.

The maze *A* is an aluminum plate, about 1/4 inch thick, cast from a pattern and subsequently smoothed and the slots filed. This plate is set upon projections in a wooden box *B*, and held in place by four lugs *C*. It is removable and may be turned through an angle of 180 degrees, or it may be turned over and used in any one of those two positions. This permits, therefore, its being used in any one of its four patterns. About 1/16 of an inch below the plate is a hard-wood slide *F*, which projects beyond the main part of the box in a handle or pull *E*. The slide can be removed and inserted easily and a sheet of paper used thereon. Below the slide are two eccentrically pivoted brass rods *D*, which are turned by the knurled nuts (also marked *D* in the plan) at the side of the box away from the subject. A third of one turn of these eccentric rods forces the hard-wood slide *F* against the lower face of the maze plate *A* and holds the record paper firm and unwrinkled when the pencil is moving over it.

In the use of the maze, a piece of newsprint paper of the same size as the slide is placed upon the latter, the thumb nuts are turned so that the paper is close to the maze plate, and the blindfolded subject begins to

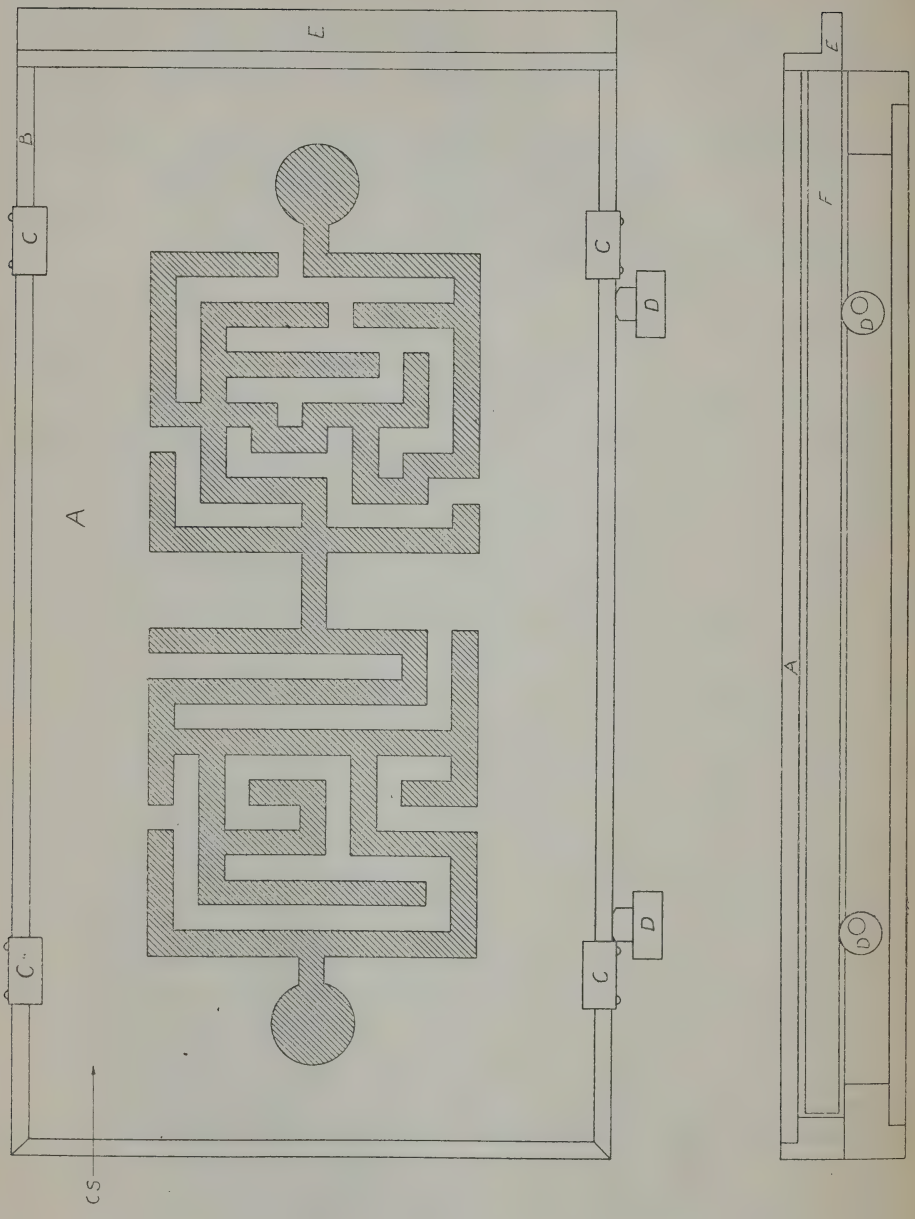

FIGURE 1

PLAN AND VERTICAL SECTION OF PENCIL MAZE

trace his pathway with a pencil. All of the correct movements and all errors are recorded, and unless the pencil is too blunt or too soft, or the subject retraces paths more than six to eight times, each effort is legible, and they are calculated after the individual experiment (or a series of experiments) is finished.

If it is desirable to exhibit general differences in the course of the individual's learning, or individual differences, to a class of about 30 students, they are evident on inspection of the records without calculation.

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THE POSITIVE GEOTROPIC ORIENTATION OF AN ANT (*CREMATOGASTER LINEOLATA*)

The "muscle tension theory" of geotropism is found applicable to a growing number of diverse organisms (4) and the present paper confirms the a priori possibility of the rational success of this conception when extended to the study of the higher Hymenoptera. The prevalent illusion that a tropism is intrinsically "primitive" is quite unpardonable, for the first quantitative expression of geotropism was obtained in a mammal (5). The behavior of ants has elicited the curiosity of mankind from time immemorial, but the burdened literature affords not a single contribution of a thoroughly quantitative character.

No review of previous studies on the conduct of ants will be found in this paper, for the results of other investigators have been invariably obscured by notions regarding the alleged "psychic" factors. We have not as yet observed any indications of a psychic life of ants and we shall refrain from postulating such factors for we have adopted Wasmann's (8) philosophy, *"Entia non sunt creanda sine necessitate,"* although perhaps not quite as Wasmann intended. Loeb (6) recognized the phototropism of the winged adult ants, but with curious inconsistency he ascribed "associative memory" to the workers and was unable to obtain a single tropistic response with them. It is difficult to detect evidence of geotropism in the published accounts of the inevitable "learning" experiments, but those of Turner (7) require comment.[1] Turner "trained" ants to descend inclined strips of cardboard (3 x 30 cm.) to reach the nest;

[1]It would be impossible to do justice to the appalling amount of anecdotal literature pertaining to ants. One example will suffice. R. de Gourmont watching ants crawling on a vertical surface concluded that these creatures were too small to be influenced by gravity (2)!

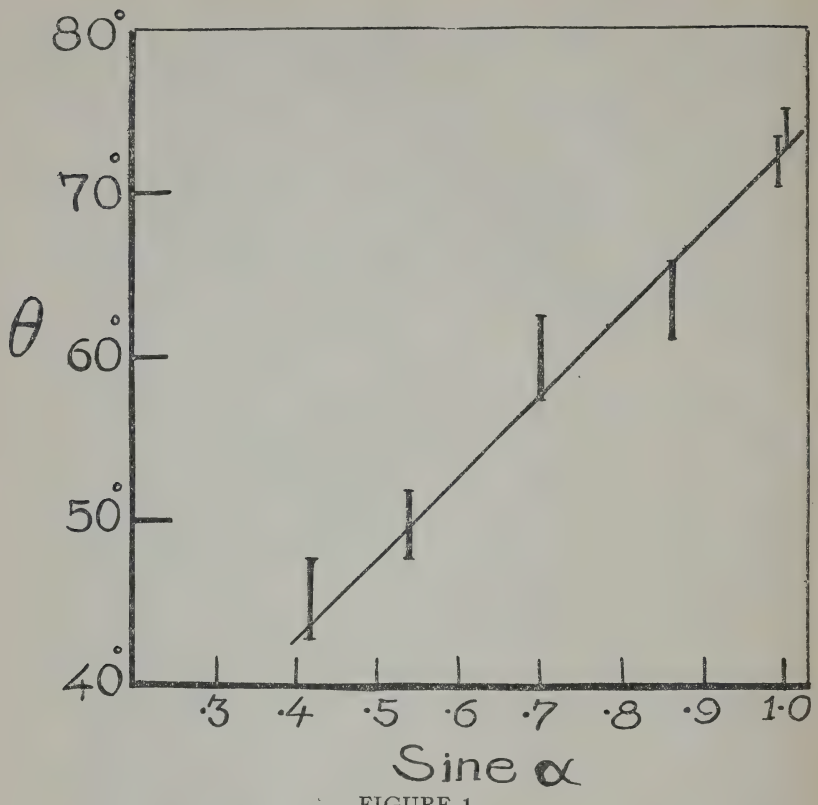

FIGURE 1

RELATION BETWEEN THE MEAN ANGLE OF ORIENTATION Θ AND THE SINE OF THE ANGLE OF INCLINATION OF THE SUBSTRATUM α

The bars are proportional to the probable error. Each value of Θ represents the mean of 40 measurements.

then, on raising the nest, he found that the animals "often took an unusually long time to learn the way home up-hill." Cornetz (3, p. 144) in his fascinating little volume on the explorations of ants proposed a most extraordinary theory to account for the path described by an ant when traversing a roof-shaped obstacle. The ant maintains the same Θ angle (the α angle of Cornetz) on the incline and decline in order to preserve its "direction"; the ant is, as Cornetz (3, p. 145) puts it, *"comme lancée en droite ligne."* The successful repudiation of this untenable hypothesis demands a metaphysical consideration of the nature of absolute space, which is unfortunately irrelevant in a modern scientific journal. The

FIGURE 2

REPRESENTATIVE RECORDS OBTAINED BY FOLLOWING THE MOVING ANT WITH A SOFT PENCIL AT VARIOUS INCLINATIONS OF THE SUBSTRATUM

A ten centimeter scale is given at the left. The light straight lines are the fitted slopes. The path begins at *X*. *A*, α 90°. This represents the nearest approach to a rectilinear trail. *B*, α 82°. The first observation made with individual No. 1 (used throughout the experiment). This "eccentric" creeping soon vanishes. *C*, α 25°. The path exhibits wide oscillations, but the positive geotropism is quite evident. *D*, α 45°. A path intermediate between *A* and *C*.

TABLE 1

ORDER IN WHICH THE RECORDS WERE MADE FOR EACH OF SIX INCLINATIONS OF THE PLANE (α)

α	Time
82	Few readings April 4th, 10 P.M.; others April 5th, 8 P.M.
90	Immediately following
45	April 5th, 10 P.M.
33	Immediately following
60	April 6th, 8-9 P.M.
25	April 6th, 11 P.M.

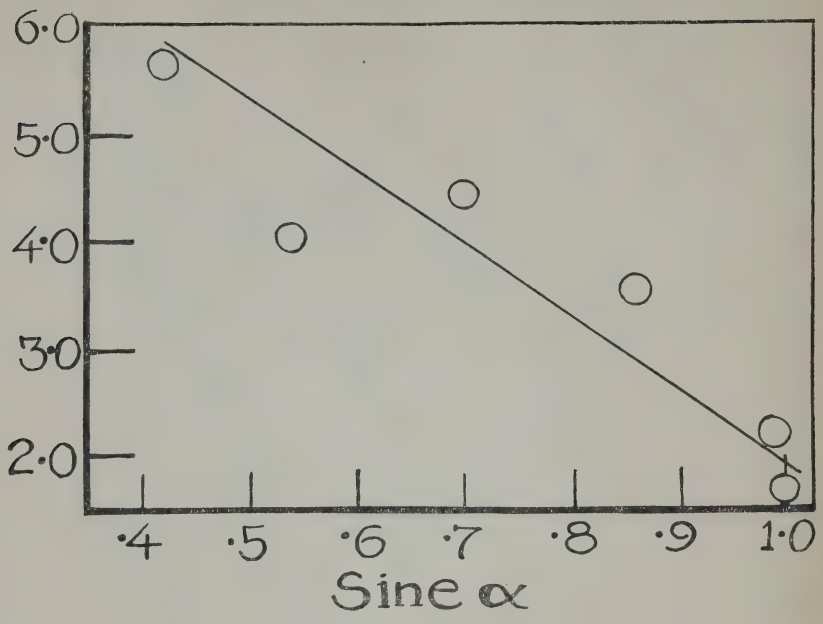

FIGURE 3

THE RELATION BETWEEN THE "COEFFICIENT OF VARIATION" AND SINE α

The ordinates represent the probable error as per cent of the mean. The variability of the angle of orientation decreases as the active gravity component increases.

splendid monograph by Brun (1) on the space orientation of ants, while dealing especially with factors in the alleged "homing instinct," presents valuable data pertaining to the *"kinaesthetische Engramm."* Unfortunately, the inclined table used by Brun contained the nest and a source of honey, but his evidence clearly demonstrates that *Formica rufa* is responsive to an inclination of the substratum as low as 20°.

The workers of *Crematogaster lineolata* whose behavior on an inclined plane is described in this paper were collected at Forest Hills, Massachusetts, in October, 1928, and kept in a large colony (with winged virgins) in the laboratory. House flies and honey were supplied occasionally. The experiments recorded were performed on the fifth and sixth of April, 1928, in a dark room under a dim red light at a temperature of 22°C. The ant was followed on a sheet of paper with a soft pencil. The texture of the paper enabled the animal to crawl readily even at 90° inclination. Immediately after leaving the colony for the first trail a worker may exhibit very eccentric crawling (Figure 2B), but when next placed on the inclined plane a typical Θ value is obtained. The ant is lifted gently to the center

TABLE 2

MEAN VALUES OF THE ANGLE OF ORIENTATION Θ AT SEVERAL INCLINATIONS OF THE SUBSTRATUM α

$n = 40$

α	Θ	P.E.	P.E. x 100 / mean
25	45.3	2.57	5.67
33	49.9	2.04	4.08
45	60.0	2.65	4.41
60	63.5	2.25	3.54
82	72.2	1.69	2.20
90	74.2	1.25	1.68

of the plane on a bit of paper and permitted to crawl off in any direction. In most cases the animal creeps downward after a few oscillations (Figure 2D). It is impossible to record every minute turn with the pencil, but the paths are accurate enough to show the gradual decrease in amplitude of the oscillations as the gravitational pull increases. In Table 2 will be found the mean values of the angle of orientation Θ measured as deviations from a line at right angles to the longitudinal axis of the plane; α indicates the angle of inclination of the substratum.

Each Θ is obtained as the average of 40 measurements of short sections of the path; the method of obtaining this is indicated in Figure 1, A, C, and D. The order in which the records were taken is shown in Table 1, for studies of *Aphaenogaster* have demonstrated that previous crawling on the plane may increase the Θ value.

It will be seen in Figure 1 that a rectilinear relation obtains between the angle of orientation and the gravitational component or sine of the angle of inclination of the plane. The angle described by the positively geotropic ant is determined by the weight of the insect, producing tension on the upper side of the supporting musculature; this tension gives rise to proprioceptive effects leading to enhanced activity chiefly in the limbs of the opposite side. Thus any deviations from the typical Θ tend to be checked until the difference in the "pull" on each side of the body is reduced below a threshold value. The probable error of Θ (from Peter's formula) and the "coefficient of variation" decrease as the gravitational component increases (Figure 3).[2]

It would be premature to elaborate a theory of the homing of ants based

[2]The precision of the orientation also increases in quite another way; the amplitude of the oscillations of the path decreases, which may be regarded as the elimination of "random movements." Further results with *Lasius flavus* will be considered in another paper.

on the demonstrated occurrence of geotropism; our task is the more humble one of showing that geotropism occurs in these insects and may be expressed in a quantitative manner. Wheeler in his monumental work on ants (9, p. 515) points out the significance of a tropism reversal but adds with characteristic sagacity, "But this is, perhaps, too simple an explanation."

Summary

The worker of the ant *Crematogaster lineolata* is positively geotropic. This gravity response may be expressed in terms of the "muscle tension theory."

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A MECHANICAL INGENUITY TEST

The following device has been applied as one of a battery of tests for mechanical skill by the Department of Psychology, Stellenbosch University, South Africa.

The apparatus consists of a puzzle box and a blue-print drawing of its interior. Figure 1 shows the external appearance of the box, which is constructed of teak wood, 1/2 inch thick. The box measures 8 by 7 inches, and is 10 inches high. The fittings, to be described, are all of brass and strongly made so as to withstand fairly rough treatment.

The inner construction of the box can be seen in Figure 2. The rods and levers are all 3/10 of an inch in diameter. As seen from the outside, the

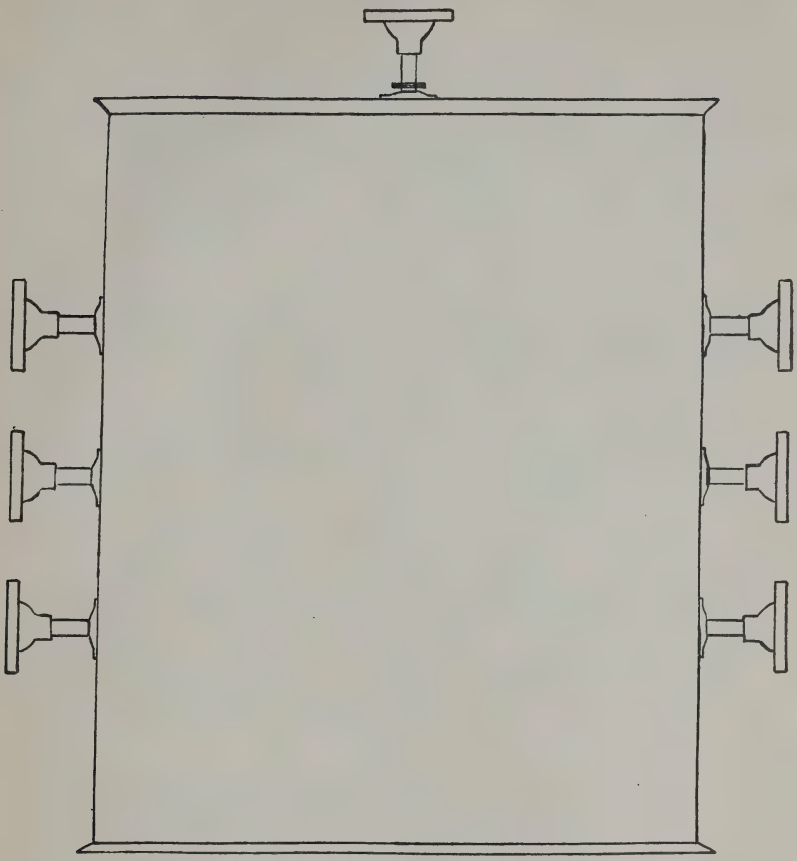

FIGURE 1

controls, with the exception of control *AH,* all appear exactly similar, and stand out 1 and 3/10 inches from the side of the box. Control *AH* differs in being fitted with an extra brass washer at *T*. All three controls on the right and left sides of the box are equally spaced perpendicularly below one another at 2 inches apart, the lowest ones being 3 inches from the base of the box. Further, they are all fitted with circular screw heads, which can unscrew if an *S* attempts to turn a control which should receive other treatment.

The rod *AH* passes through the lid and screws into the seating *R* at *H*. *Q* is a ring of 1-inch diameter, which forms part of the rod *AH*. Through this ring passes the rod *DG,* which consists of two sections, *DP* and *PG*.

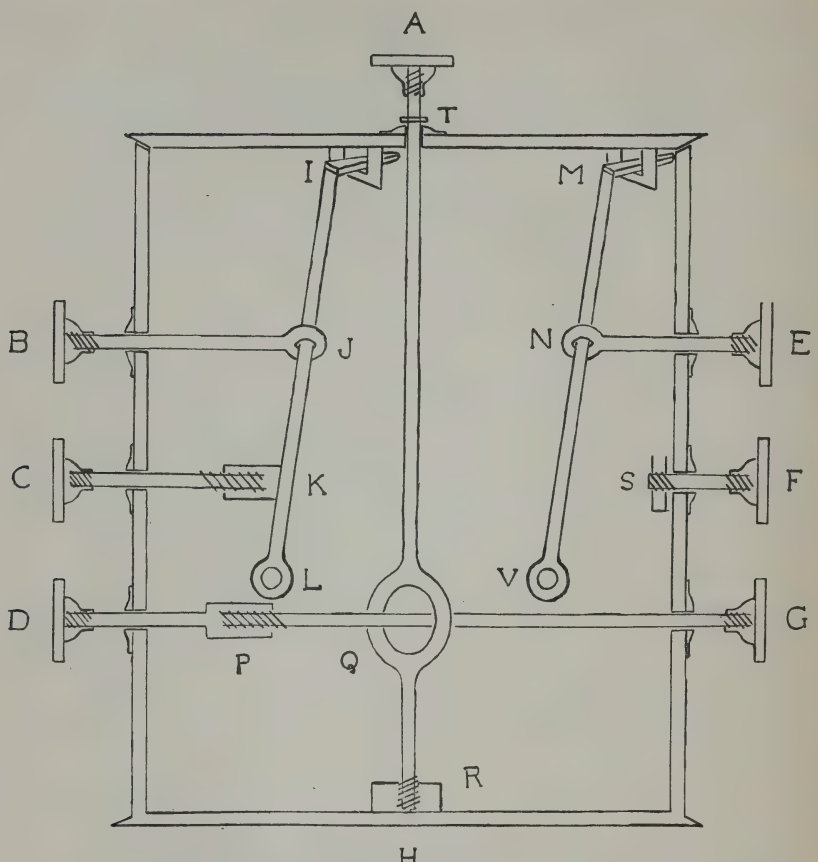

FIGURE 2

The control *DP* has a screw seating at *P*, while the rod *PG* is fitted with a screw end at *P*. When *DG* is fixed in position, it effectually locks the control *AH*, while the washer at *T* prevents the alternative of unscrewing the screw head at *A* and slipping the lid over the remaining rod.

Attached to the lid are two slots, *I* and *M*, set parallel to the sides of the box, but depicted as in Figure 2 in order to make the essential mechanical relations involved more easily comprehensible from the drawing. Slot *I* is fitted 1 and 9/10 inches from the left side of the lid; slot *M* is 5/20 of an inch from the right side. The catches of the levers *IL* and *MV* fit into these slots when the box is closed. The lever *IL* turns on a swivel at *L*. Similarly, the lever *MV* turns on a swivel at *V*. Both these levers are

5 inches long, while the catches at *I* and *M* measure 1 1/2 inches. The lever *IL* can be manipulated from outside by means of the controls *BJ* and *CK*. A screw seating is welded on to the lever at *K*, into which the rod *CK* screws. The control rod *BJ* is fitted with a collar at *J*, in which the shaft *IL* moves freely. In the same way *EN*, the control rod governing the lever *MV*, is fitted with a collar at *N* through which the shaft *MV* passes. The swivels at *L* and *V* are situated 1 and 8/10 of an inch from the side of the box nearest to them. The dummy control *FS* is kept in place by a lock nut at *S*. It is so attached that the whole fitting can turn freely.

This closed box, as described, is placed before the subject, and it is explained to him that certain catches inside the box keep the lid fixed in position, and that these catches are controlled by the levers projecting above and on each side of the box. The subject is then given a schematic blue-print drawing of the box seen in cross-section (as in Figure 2), and he is instructed to open the box as quickly as he can. By referring to the blue print, which remains before him during the test, he is able to ascertain what the mechanical relations are between the levers and the catches.

The time taken to carry out the test successfully serves as criterion of the *S*'s mechanical ability; secondly, the number of erroneous handlings of the levers, as described below, afford a further measure of efficiency.

The correct procedure in opening the box is as follows:

1) While lever *D* is held fast in the left hand, *G* must be unscrewed, and then drawn out as far as it will go. The apparatus is so made that, if the subject omits to prevent *D* from moving, the entire shaft *DG* will turn when the attempt is made to unscrew *G*.
2) Next *C* must be unscrewed and drawn out. This operation unlocks *BJ*,
3) which can now also be drawn out.
4) *E* must be pushed in flush with the side of the box.
5) *F* remains untouched (being the dummy).
6) Finally, *AH* must be screwed loose and the lid lifted.

It will be seen from the illustration that the order given need not be adhered to in all respects but, also, that the different steps cannot all be carried out in any order. When *CK*, for instance, is still in place, *BJ* is effectually locked. Similarly, the shaft *AH* is locked by *DG*.

Under erroneous attempts is included:

1) Premature attempts to force the lid off.
2) Manipulation of the dummy lever *FS*.
3) Attempting to turn control *BJ* or *EN*.
4) Attempting to draw out control *EN* or push in *BJ*.
5) Attempting to unscrew *AH* prematurely.
6) Attempting to draw out *BJ* prematurely.
7) Attempting to force levers *AH*, *CK*, or *DG* in or out instead of unscrewing them.

One hundred and sixty boys, averaging 14 years of age, were tested with this apparatus at the Johannesburg Trades School. The test was carried out in the following way:

1) First attempt—instructions were given to open the box with the blue print as reference.
2) Immediately after the successful completion of this task, the *S* was instructed to reclose the box with the blue print as reference.
3) Thereupon the *S* opened the box again, now without reference to the blue print.
4) The *S* had finally to reclose the box without the help of the blue print.

It will be seen that in the reclosing of the box, the order of the different operations is reversed.

AVERAGE TIMES TAKEN

1	4 min. 5 sec.	First application
2	3 min. 16 sec.	Second "
3	1 min. 7 sec.	Third "
4	1 min. 45 sec.	Fourth "

Ninety of these boys have recently written a Government Preliminary Technical Examination. This test is mainly theoretical but includes practical geometry and drawing. The other subjects examined are: language, trade theory, chemistry, mathematics, and physical science.

If we divide these 90 boys into two groups, according to their success or non-success in the examination, we find that the average time taken by the first group for the opening of the mechanical test box is 3 min. 50 sec. The second group's average time is 5 min. 27 sec.

The teacher's opinion regarding the boy's workshop ability shows with the first application of the test a correlation of .36. Although this result is not as satisfactory as one would like, it is partly due to the fact, we hold, that the boys had not all been at the school for the same length of time. Further, it was found that the type of work done at the school did not in all respects incur the particular kind of mechanical insight presumably tested by this test.

The further applications (2, 3, and 4) gave no agreement with examination results or the teacher's judgment of the boy's ability. Those boys who took over 20 minutes in opening the box the first time take a very short time in reclosing it; others who spend a very short time in opening the box take a longer time to reclose it. This is due, we hold, to the fact that those who take a long time on the first form of application had, in so doing, a better opportunity of studying the apparatus, and obtaining a better knowledge of its internal arrangement.

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A UNIVERSAL FINGER MAZE: THE PATTERN BOX[1]

Maze Pattern and Maze Construction

At this stage of development when maze work itself has hardly come out of its own blind alley, it is highly profitable to distinguish *maze pattern* from *maze construction.* The difficulty of a maze is not only a function of the pattern but also of the material or manner in which it is constructed. While the controversy over the reliability of maze work (4, 1, 3) has resolved itself into a question of pattern (10) and statistical considerations (11, 12), the influence of construction upon the results of learning can hardly be overemphasized (7, 5). Two types of construction for all manual mazes for humans are now available, namely, the old depressed stylus or groove type and the new elevated "finger" type (7, 8). The difficulty of construction,[2] the objection of running along one side, and the cost of construction will surely yield the former to the latter for general usage. Vincent's innovation (13) of taking away the walls of the usual alley type for studying the function of vibrissae in the behavior of the white rat might have been the forerunner of the later elevated type of construction, but it was through the deliberate effort of Professor Miles that the promised elevated type of construction as a new technique was definitely established.

To be sure, this elevated type of wire construction is much more readily made, can be easily duplicated, lends itself to the making of changes, can be more complex, and, above all, can be traversed much more rapidly than the former depressed type of groove construction. But with the *discrete-path* variety described in this paper, by the use of shots, nails, balls, and pins on a perforated metal screen or a wooden board, any pattern can be built or destroyed at will without disturbing the unit itself, and hence it is *universal* in nature. In comparison with this, the wire construction is much less readily made, can be much less easily duplicated, lends itself much less to the possibility of making changes, although perhaps equal complexity and rapidity can be obtained by it. The readiness for making

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[1]Expenses in connection with the developing of the *pattern box* were met from the Thomas Welton Stanford Fund for Psychological Research. The writer is indebted to Professor W. R. Miles for criticisms and suggestions.

[2]In connection with another development called the multiple groove board described in (2), which is essentially a series of vertical and horizontal, machine-milled grooves, and is designed for testing manual skill in controlling a rolling ball along prescribed grooves, a simple method for constructing a groove maze was suggested. It consists of nailing pieces of wooden blocks on a panel base along all the boundaries of the maze pattern so that walls are formed by the blocks on each side of the path. Professor Miles has actually adopted this method of constructing the groove maze (6) in connection with a comparative study between the stylus and the finger maze.

FIGURE 1
THE PATTERN BOX: A UNIVERSAL, DISCRETE-PATH, FINGER MAZE BOX

a pattern in the wire construction is perhaps not so obvious as it first seemed to be in description (7). The panel has to be, first, specially cut and sandpapered; the design or pattern has to be marked accurately and proportionally by pencil on the surface before the wires can be nailed into the marked units. The cutting of the wire requires a special cutter and some previously trained skill, not to say the delicate judgments of the different lengths of the units and the careful bending of the two ends for engaging into the wood. No less skill is involved in the hammering of the units into the wood. After the different units are completely inserted, the entire surface has to be varnished to prevent it from getting dirty through constant tracing by the finger. Thus at least one day is required to build any one pattern, and duplication means, of course, going through the entire procedure once again. Once made, the pattern practically lends no possibility to make changes (contrary to what the first description (7) seemed to contend because the removal of any unit would certainly leave a hole at each end and hence make the floor not uniform.

The writer's interest in the development of a discrete-path, universal-construction variety of the elevated finger maze began in the fall of 1926 when he first saw a demonstration by Professor Miles of this type made from narrow square oak strips glued to a panel. The writer was aware of the possibility of such a variety and was convinced then that, when the old Hampton Court diagram was first used by Small (9) as a rectilineal maze for animals, probably it never occurred to anybody that "maze" would become synonymous with animal learning, that maze work itself has come to its own blind alley, and even its usefulness as an instrument of research in animal learning (4, 1, 3) has been doubted, that this doubt cannot be cleared unless standardization of the apparatus (14) is perfected and evaluation of the merits of different maze patterns (10) is carried out, and that its positive value and usefulness as an instrument of research on behavior of both human and infra-human subjects cannot be established unless some convenient piece is devised that is really easy to set up and universal in nature so that a comparative study of the different existing maze patterns can be made on human subjects to ascertain their relative merits all at once. The *pattern box* together with some related varieties of universal, discrete-path, finger maze-construction described in this paper was a direct outcome of this awareness and conviction.

Some Varieties of Universal Maze-Construction

The writer started with the original idea of using small ball bearings of 3/16″ diameter to make the maze pattern on a piece of 3-ply panel of white cedar, in which a series of holes of 1/4″ diameter in the shape of a half bowl was drilled just deep enough to hold the balls in position. When not in use, the balls were kept in their respective holes, making the maze pattern by a sheet metal cover. As holes were in rows and columns all over the board, any vertical or horizontal path could be formed instantly.

This is the *first* design. A *second* design was to drill the holes from the bottom of the board but not quite clear through with the drill so that the balls may be inserted from below and kept inside the holes by a piece of thin metal plate or wooden board placed below. This will keep the maze pattern relatively permanent because the balls can neither fall off from above nor slip off from underneath. A *third* design was to drill two duplicate plates of tin sheet 1/16″ thick exactly alike, the diameter of the holes being just a trifle smaller than that of the ball so that when the maze pattern is made the two plates will keep all the balls between them by bolts on the four corners. This design gives the extra advantage of obtaining the mirror effect of the maze pattern by simply turning the whole apparatus over and using the back pattern.

The difficulty of drilling the holes, either in the wooden board or in the metal plate, small and close enough to make the path reasonably continuous and the failure to find some ready-made material that will fulfill the requirement led the writer to make use of ordinary brass-plated upholstery nails. The first thought was some sort of tack with a round smooth head and a stem; then Professor Miles suggested this particular nail, which is about 14 mm. long with a round smooth cap of about 8 mm. in diameter. Double corrugated cardboard and 1/4″ plain linoleum were tried for the base of the maze pattern. The nails can be pushed in just like thumb-tacks so that building or destroying a maze pattern is a matter of minutes. The appeal of this design is its simplicity and cheapness of construction besides the universal nature of maze pattern.

These designs were all more or less preliminary experimentations to the perfected universal, discrete-path, finger maze-construction called the *pattern box*. The name was chosen because of its many possibilities both as a psychological apparatus and as an educational instrument.

The Pattern Box

After quite a little further search for a better material than the brass upholstery nails and for some ready-made perforated plate for the pattern base, the usual commercial brass escutcheon pins (size No. 14 with a head diameter of about 2 mm.) were found best for the purpose together with the ready-made perforated tin sheet that is used in all metal works for housing instruments that liberate considerable heat. The perforated tin sheet comes 14″ x 20″ at the cost of about 60 cents a piece (retail price). Any size of holes may be obtained. As the perforations become larger, the distance between them becomes proportionally greater. For No. 14 escutcheon pins, a plate with ten perforations to the inch just fits.

It is obvious that any number of universal, discrete-path maze-boxes can be very conveniently made by putting a wooden frame around the four sides of the perforated tin sheet with a sliding cover to keep the pins in position and be used in each laboratory over and over again, thus avoiding

the trouble of building a separate maze board for every maze pattern as in the case of wire construction. The discrete path formed by the pins on the perforations has all the characteristics of the wire path and can be altered instantly. A special pin picker bent out of a piece of flexible brass makes it possible to pick a row of pins out of the perforated screen all at once.

The *pattern box* was an elaboration of construction of this sort of universal, discrete-path, maze box. As shown in Figure 1, the whole box is a neat, portable, flat case, which measures 19½" x 20½" x 1⅜". It was designed to be used on both sides. The two panel covers (*C*1, *C*2), which slide easily along the grooves just like an ordinary box cover, here serve four distinct purposes; namely, as covers for the box, as protector of the patterns on the screen *T*, as bottom for the six compartments for storing pins and other accessories, and as a screen for keeping the pattern for experiment away from the view of the subject as shown in the position of *C*1 by the aid of two S-shaped hooks. When not in use, the two covers are locked to the box by the S-shaped hook *H* and keep both the pattern and the accessories all securely inside the box. The extra inner frame *F* serves both as the fourth side-frame for the perforated screen base and as the other frame for the compartments.

On the screen are shown some of the illustrative sample patterns. The modifiability and appeal of the whole design can be appreciated from the fact that designs or patterns as small and complex as the letters of the alphabet can be built on the screen. The big size upholstery nails with rough or smooth heads were used as the start or finish of a maze pattern.

One exceptional advantage of this universal, discrete-path maze-construction over the panel-board, wire construction is that it lends itself also to the possibility of using the same kind of wire which Professor Miles uses in building continuous path units by engaging the two bent ends into holes wider apart on the screen. A convenient and neat arrangement was to cut ordinary Gem paper clips No. 1 in two and to hook the two ends of the staples thus made out of the resulting halves on the perforations of the screen.

As the pattern box is perhaps too elaborate for ordinary use, some suggestions for modified forms are here given. (1) A single sheet of the perforated screen can be very conveniently and cheaply made by furnishing four frames and a cover. (2) The cover and protector can be made to swing upon a hinge, like opening the leaves of a book, instead of sliding along the grooves. (3) The perforated screen can be made separate and detachable so that different ones with different sizes of perforations may be used as desired.

The possibilities of the pattern box are many. It can be used for picture-completion tests and performance tests as suggested by Terman, for spot

pattern tests by McDougall, for tactual experiment on Gestalt by Koffka, for a toy for children, and for the use of blinds by Miles, although Tolman was content to use and own it for general maze experiments for which indeed it was originally designed. It may be used also as a motor test. Artists, designers, and blind readers perhaps can make great use of it. At the suggestion of Professor McDougall, the Stoelting Company has been approached to put it on the market.

The writer anticipates with keen interest that, with the wire technique of maze construction as originated by Professor Miles and with the universal, discrete-path construction as described in this paper, maze work will take a new direction to throw new light on the validity and reliability of the "maze" as an instrument of research on both human and infra-human subjects.

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FEELING AND KNOWING

In surveying the current psychological literature, including both articles and textbooks, one is impressed by the remarkable longevity of the age-old antithesis between feeling and knowing. Remnants, even more than remnants, of the faculty psychology are still with us. It cannot be said of this particular speculation that the good die young. It is not young, and it is not dead. It was at first probably a semilogical distinction before the days of stimulus and response, process psychology, controlled introspection, and behaviorism. The fact that it now lingers with us is evidence that even psychologists are quite human and, like other humans, are frequently the followers of tradition. As we grow older, changing fashions appeal to us less and less, for there are fashions in science as well as in clothes.

From the exclusively introspective aspect there may certainly appear to be differences, but, if we view behavior in an objective way, without becoming technical behaviorists—if that is a sin—then it is apparent that the only real difference or distinction between feeling and knowing, to use a trite expression, is one of degree, not of kind. The difference or differences must be expressed ultimately in terms of quantity, not of quality, for all scientific explanations must be in quantitative terms, meaning by that in terms of mathematics. All qualitative differences are in the last analysis quantitative differences. This does not mean the unreality of the qualitative, but that there is, so to speak, a common denominator enabling us to make measurements and comparisons.

Feeling represents a vague, undifferentiated, undefined, indefinite, and primitive type of response to a rather indefinite, vague, hazy, and unanalyzed situation. Knowing, on the other hand, in the sense in which this term is used, is simply a more detailed behavior of response. It is more definite, less vague, less primitive, more adequate, and all this is made possible by more detailed and more complex structures as found in the receptors, conductors, and effectors of organisms which have reached a certain stage of development. Thus these organisms are able to analyze their environment. The more complete the analysis, the more exact the adjustment in the form of response, in other words, the more complete the knowing. Knowing and feeling, therefore, are to be compared in terms of behavior, measured in terms of amount. There is a gradation of responses from what we call feeling through instinct or emotion until we reach knowing and what is called rational behavior.

This position is in harmony with the action theory of consciousness so well expressed by Münsterberg. It is in agreement, too, with the biological conclusion that the higher or newer structures include the lower or more primitive, and also with that theory of instinct which states that the drives are protopathic and the expression epicritic, with the additional qualifica-

tion that feelings are more primitive than emotions and instinct.

If there is some truth in this position, then there is an explanation why feelings accompany or form a part of all other forms of knowing and why some psychologists have listed them as sensations of an elementary sort. Even the mere attraction to a stimulus or withdrawal from it constitutes an elementary form of knowing which, as we know, accompanies the higher process, which is traditionally called cognition. It may be, too, that between these two extremes a place can be found for what is called insight and sagacity.

The order of precedence, then, would be something after this fashion—feeling, emotion, cognition, on the so-called subjective side; and attraction-repulsion, instinct, rational behavior, on the so-called objective side. As further evidence that the higher forms of knowing include the lower, consider human behavior in times of crises, emergencies, etc. There is a reversion to the lower when no other or higher appears to fit the situation. Perhaps it is incorrect to speak of higher and lower. Simple and complex would probably be more adequate terms. Thus the primitive is always with us. In all forms of knowing or behavior, feelings and emotions are present. They are part and parcel of all knowing and there is really no antithesis, as some belated thinkers would have us believe. Knowing is thus a rather homogeneous process to be taken as one whole, and the various stages are to be regarded as differing more or less in complexity.

Feelings and emotions are thus simpler forms of cognition corresponding to simpler response systems, which, of course, as we know, are included in the higher response systems. If we acknowledge the biological principle, how can we exclude the psychological, if structure and function go hand in hand through the ages? All of our behavior can really be divided into two classes: (1) behavior towards a stimulus; (2) behavior away from a stimulus. Our behavior in the intellectual sense is nothing more or less than an extension of the more primitive forms. There is more manipulation of environment due to more complex structures in receptors, conductors, and effectors. In kind it is not different from the primitive, and should not be held in contrast as if it were something different. Knowing belongs to the same continuum as feeling.

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ONE HUNDRED CASES OF COLOR-BLINDNESS DETECTED WITH THE ISHIHARA TEST[1]

For two years, 1927 and 1928, at Stanford, we have used a color-vision test as a routine part of the physical examination of entering students, athletes, and others who come under the scrutiny of the Department of Physical Education for Men at the beginning of the autumn quarter. The number examined each year has been around 600. The examinations have been concentrated into four days, which means that when the system is working smoothly the men come along with their cards at the rate of about 25 per hour. If one examiner is to handle the color-vision test, he has two or three minutes to devote to each man.[2] By using the very cleverly devised Ishihara Test (6),[3] which consists of numbers in colored spots on a color-contrast background of colored spots, it is quite feasible to examine men at this rate and to secure trustworthy results on them.

We do not know just when the Ishihara Test became available for use in America or how widely it is known and used at present. Dr. Janet Clark described it accurately and fully in 1924 (4) and designated it as "the most successful of the various colorblindness tests." She compared the Edridge-Green card test, Stilling's plates, and the Ishihara plates. All three are similar. The first two are based on the fact that numbers printed in red or green and without distinct borders cannot be distinguished on backgrounds of browns, tans, and grays by the red-green blind. Therefore on Stilling's plates, for example, if the color-blind fails to see the green or red number on a plate he usually sees nothing, or perhaps guesses a wrong number from fragmentary cues. Dr. Ishihara, while making use of this same fact in his plates, designed them to take advantage of the further consideration that blue, yellow, and violet for the usual color-blind are remarkably bright as compared with red and green colors. In several of the Ishihara plates there are two numbers superimposed. The normal eye sees the green or red number, while the red-green blind fail on this but read confidently and promptly the (to the normal eye) very faint violet or blue number. Chase (3), 1926, in his discussion of methods and problems concerning color-blindness and industry, states that he has reviewed examples or descriptions of more than one hundred tests. He does not mention Ishihara, although giving attention to Stilling's plates and

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[1]Acknowledgment is made to Dr. Thomas A. Storey, Director of Physical Education for Men, Stanford University, for his kind cooperation. He informs me that the results of this work have convinced him that it is desirable to use regularly a color-vision test in their routine physical examination.

[2]It is best to make examinations by daylight between 9 to 12 A.M. and 1 to 4 P.M.

[3]Devised by Dr. Shinobu Ishihara, Professor of Ophthalmology, Medical Department, Imperial University of Tokyo.

holding that these should be used in conjunction with the Edridge-Green lantern as the standard test for color-blindness. He states further, "Stilling's plates are the standard of this group, and of great use in convincing officials, and friends of the candidate. They fail to detect color ignorance, small scotomas, and some forms of blindness, including shortened spectrum." We are inclined to think that Chase had not discovered the Ishihara Test; however, his criticism probably applies to this general type of test. Dr. Ishihara in the introduction to his test manual states that his plates are "intended to discover quickly and accurately congenital colorblindness" and that "acquired colorblindness, being of secondary significance, has been disregarded." There are, however, ways of giving these reading tests for color-blindness, e.g., varying distance, requiring color naming of the numbers, of the backgrounds, and of particular dots, and testing one eye at a time, which will bring out more than the mere fact and nature of congenital defect. But as a means of quickly and accurately discovering the latter among literate adults the Ishihara Test leaves very little to be desired.

The plates are mounted on a folded strip, which can be turned as pages of a book or stretched out and fastened up for horizontal display of the entire series. This full-display method recommends itself when a large group is to be examined. Sibyl Terman (8) used the test in this manner for selecting the 63 cases of color-blindness which she studied more intensively by other methods. The Ishihara plates were all displayed to a class of 20 or 30 university students. Each member had a mimeographed form on which to write down the numbers which he or she read from the charts. Distance was not constant, and doubtless some people were influenced by others who could make out the numbers more promptly. It is not clear how Haupt (5) used the Ishihara Test, whether as a book or with all the charts displayed. She studied 487 girls and 448 boys and gave it individually and also monocularly, but the distance is not specified.

In our own work the group of plates was attached to the wall or to a vertical screen at a height of about 5 feet and under good even daylight illumination. Two chalk lines were made on the floor, one at 4 feet, the other at 10 feet from the vertical plane carrying the charts. The men were admitted to the room one at a time and were tested binocularly. They passed along the 4-foot line parallel to the plates, reading them off one after the other. In each case, therefore, the observer was about squarely in front of the plate to be read and about 4 feet away from it. If he made no mistake or only one mistake, he was judged normal and no further testing was done. If he made several mistakes, we took his name, started him again at the first of the series, and wrote down his response to each plate. Then in many of the cases, although this was not carried out as a regular part of the procedure, the observer was asked

to step back to the 10-foot line and to make sure at this distance whether the numbers were clearer, less clear, or unchanged in their appearance.

The numbers on the Ishihara plates are about 2 inches high, a trifle more when there is but one number on a plate and a little less than 2 inches where there are two numbers on a plate. At a distance of 4 feet a number 2 inches high subtends a visual angle of about 2½°, while at 10 feet it subtends 1°. The fovea is commonly considered to have a 1° diameter, and that of the rod-free area is supposed to be close to 3°. A subject with normal color vision can easily read the plates at a distance of 10 inches, where the numbers would subtend about 11½°. If a subject fails on several of the charts at 4 feet but can read them or see the numbers much more clearly at 10 feet, it may be supposed that his color fields are extremely limited, being practically confined to the fovea.

With the aid of several assistants[4], who have helped or have conducted the examination on different days, 1286 Stanford men have been individually examined. In order to be considered a case of defective color vision, a subject had to fail on or misread two or more of the plates. There were 106 such cases, which amounts to 8.2 per cent of the group tested. Approximately one-half of these cases were found to be unaware of any color-vision defect. The same test given in the same way[5] was administered to 436 Stanford women. Out of this entire number we found one case of undoubted color-blindness. Her father also was color-blind, and she discovered her own condition at the age of 12. She misread or failed on 8 plates, but 65, that is, 61 per cent, of the color-blind men made more mistakes than did this color-blind woman. There were three other women in the 436 who made one mistake each. In every case this was on Plate 5, which the normals read as 74[6] and the red-green blind read as 21. Two of these cases read it as 21, the other gave 71. Color-blindness in women is so rare than even slight defect is probably significant and should be counted. For this reason in this survey we have tentatively counted 4 cases (0.9 per cent) of color-blindness or color-vision defect among the 436 women tested.

Our results may be compared with those of Haupt, who gives a table (5, p. 301, Table 7) showing the incidence of defect as revealed in 487 girls, age 7 to 16 years, and 448 boys of the same age range. Concerning cases entered in this table, it is stated that "except where indicated in

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[4]My acknowledgments are to Henry Beaumont, Kirk Miles, Harold C. Sox, and W. L. Roach, all of whom were employed at different times as technical assistants in these routine examinations.

[5]The women were examined by Mrs. Ruth T. Wilson, for whose assistance I am grateful.

[6]Clark says "This one card..... is a better test of color vision than all of the plates of the Edridge-Green and Stilling tests together" (4, p. 274).

brackets, all the plates in the Ishihara Test were missed. The table includes all cases failing more than one (the 5th) of the Ishihara plates." There were 8 girls and 35 boys who demonstrated defect in color vision and for each sex the cases were well distributed throughout the entire age range. Apparently 31 of the boys and 3 of the girls missed all the plates. Five girls missed part of the plates. One girl, 12 years old, missed 7 of the plates with the left eye but made not one mistake in the Nela Test with this eye. Another girl, 9 years old, made no mistake on the Ishihara Test with the left eye but made 18 out of 22 possible mistakes on the Nela Test with this eye. The 31 boys who missed all the plates show a remarkable variation in the number of mistakes made on the Nela Test. We have had no experience with this latter test and, therefore, cannot go further into a discussion of these comparison results.

Haupt gives a summary table of estimated percentages of color-blindness. This embodies results from many sources.[7] The examination has usually been made with wools or specifically by the Holmgren test. We may now add a supplementary table (see Table 1) of results with the Ishihara Test.

TABLE 1

INCIDENCE OF COLOR-BLINDNESS OR DEFECT IN COLOR VISION AS INDICATED BY THE ISHIHARA TEST

Examiner	Year	Material studied	Cases found	
			Number	Per cent
Haupt	1926	448 boys, 7-16 yrs.	35	7.8
Miles	1927	590 university men	48	8.1
Miles	1928	696 university men	58	8.3
Haupt	1926	487 girls, 7-12 yrs.	8	1.6
Miles	1928	436 university women	4*	0.9

*Three of these cases made only one mistake (see text) whereas all other instances in the table have made two or more mistakes.

This shows about 8 per cent incidence for men, while the typical wool test is usually credited with about 4 per cent. Similarly for women we find a higher per cent of defect indicated with the number test for color-blindness than with the wool test. Color-naming or color-matching is doubtless more simple and elemental than is the discrimination of design in color, but probably the latter is more generally important and useful. The eye in exploring the visual field usually "covers it" in unit areas considerably larger than "fovea size." We ordinarily use larger units than this even in reading. Color fields that extend well beyond the fovea are thus of great usefulness in our normal perceptions of colored objects. Defect of this character is indicated by the Ishihara Test.

[7]As recent bibliographies on color-blindness the reader may consult (1) and (2), which includes a chronological bibliography of 425 titles.

The responses of our 106 cases with the different Ishihara plates are analyzed in Table 2. At the left is the plate number. This refers to the placement in the series considered from left to right when fastened on the wall. The plates, which are 12 cm. square, are attached to a heavy strip of black paper, 19 cm. wide. Above each plate its serial number was written in white. These serial numbers were large enough to be seen at 10 feet. They aided the examiner in his work, for example, he could say, "Please read Plate 7 again." The second column shows the number on each plate that is observable to those with normal color vision. On Plates 10 and 11 the color-blind frequently see numbers, but the normal do not; for sake of simplicity we have eliminated them from the test. Plate 12 has the 2 in red and the 6 in violet-red. Also, for Plate 13 the 4 is red and the 2 is violet-red, while the background for each of these plates is gray. For convenience I have counted Nos. 12 and 13 as equivalent to two plates each. The third column in Table 2 gives the response that is usual for color-blind subjects. For Plates 12 and 13 subjects usually see the red 2 and the red 4 or the violet-red 6 and violet-red 2. The totally color-blind are not able to see numbers on any of the charts with the possible exception of No. 1. The fourth column gives the number of times that a given chart was read correctly, that is, as the normal are supposed to see it. Then we give in order the number of times that a given chart was read as the red-green blind are supposed to see it, the number of miscellaneous readings (imagined numbers), and finally the number of times no number whatsoever could be seen. The percentages are all computed on the basis of 106 cases.

From Table 2 we see that the first plate is calculated to inspire confidence at the start of the examination; everyone was able to read it. No. 2 was correctly read by half (52 per cent) of those who have color defect, and 37 per cent saw the alternate or hidden number in it, while only 3 were unable to see any outline. Somewhat more difficulty is experienced with Plate 3, which like No. 2 has a green background but more red in the number. This causes fewer to read it correctly and more to see the alternate hidden 5. There are very few who correctly read Plates 4 and 5. Here the numbers are in green and blue-green, and the backgrounds have red in them. The hidden numbers are in violet-red and blue. About 80 per cent of the cases see the hidden numbers and but very few see the plates as blanks. Plates 6 and 7 each have one red number on a green field, and 8 and 9 have green numbers on fields made of violet-red and yellowish dots. There are no hidden numbers on these four charts, and the color-blind characteristically fail to see anything on them. Quite regularly more fail to see the green numbers than the red ones. For the combination plates, Nos. 12 and 13, we get very clear-cut results. About 72 per cent of all cases promptly report the red 2 and 4. There were 48 who saw

TABLE 2

THE RESPONSES OF 106 COLOR-BLIND MEN ON THE VARIOUS ISHIHARA PLATES

Plate number	Normals read	Usual color-blind read	Read correctly		Read as usual for color blind*		Miscellaneous readings		Unable to see numbers	
			Frequency	Per cent	Frequency	Per cent	Frequency	Per cent	Frequency	Per cent
1	12	12	106	100	106	100	0	0	0	0
2	8	3	55	52.0	39	37.0	9	8.5	3	2.8
3	6	5	41	38.7	52	49.0	12	11.3	1	0.9
4	5	2	10	9.4	90	85.0	4	3.8	2	1.9
5	74	21	7	6.6	83	78.0	10	9.4	6	5.7
6	2	..	14	13.2	71	67.0	21†	19.8	71	67.0
7	6	..	22	20.8	74	69.8	10	9.4	74	69.8
8	5	..	3	2.8	87	82.0	16	15.1	87	82.0
9	7	..	7	6.6	86	81.0	13‡	12.3	86	81.0
10	..	(5)								
11	..	(2)								
12	26	2 or 6	14	13.2	2-77	72.6	15**	14.2	13	12.3
					6-23	21.7				
13	42	4 or 2	24	22.6	4-76	71.7				
					2-37	34.8	5††	4.7	13	12.3

*Number of readings that correspond to the third column from the left.
†Of these, 15 read the plate as 8, which is fairly easy to confuse with 2.
‡Of these, 11 read the plate as 9 which is easily mistaken from 7.
**There were 7 others who read it as a single number not 2 or 6; 48 read it as 2 only; 9 read it as 6 only.
††There were 3 others who read it as a single number not 4 or 2; 49 read it as 4 only; 12 read it as 2 only.

only the 2 on No. 12, and 49 who saw only the 4 on No. 13, that is, 45 per cent of the whole group were quite oblivious to the violet-red numbers on these plates. Aside from these 48 above mentioned, 15 cases saw the 2 and had a hint that there was an adjoining number, reading it as something in the twenties but not 26. Seeing one number far at the side prompts a very careful search for the other number. Likewise in the case of the 42 (Plate No. 13) there were a few who could not quite make out the 2 but guessed something in the forties. For the violet-red 6 there were 9 who got only this but 23 in all who saw it, while 37 saw the violet-red 2, and 12 saw nothing else on that plate. No. 13 is found to be easier than Plate 12 although both gave 13 cases of complete failure.

If we classify the cases according to the suggestions in Dr. Ishihara's manual, the groups are as exhibited in Table 3. Here we show also the results obtained by Sibyl Terman (7). Green-blindness is more frequent than red-blindness in the ratio of about 3 to 1.

TABLE 3

CLASSIFICATION OF THE COLOR-BLIND ACCORDING TO THE ISHIHARA TEST RESULTS

Class and condition	Miles		Terman	
	Number cases	Per cent	Number cases	Per cent
Incompletely red-green blind	14	26.9	23	21.7
Nearly completely red-green blind	5	9.6	5	4.7
Completely green blind	19	36.6	49	46.3
Completely red blind	6	11.5	8	7.6
Completely red and about completely green blind	4	7.7	5	4.7
Completely green and about completely red blind	1	1.9	1	0.9
Completely red-green blind	3	5.8	15	14.2
	52		106	

It is obvious that the Ishihara plates are not all equally difficult; however, we may consider the results for our 106 cases of color defect from the standpoint of how much of the test they could interpret correctly. Suppose we give one point for the correct reading (as the normal eye sees it) of each plate, 2 to 9 inclusive, and one point for each single digit correctly read from Plates Nos. 12 and 13, that is, two for each of these plates. On this basis a

perfect score would be 12 points. All our cases fall below this as a condition of their being in the group. For the different point scores we then have the following distribution of cases:

Point scores	0,	1,	2,	3,	4,	5,	6,	7,	8,	9,	10,	11,	12
No. of cases	6	6	36	17	13	7	9	2	4	4	2	0	0

From this it is seen that the typical case was able to read only 2 or 3 of the digits displayed. Usually this included the red 2 and 4 of Plates 12 and 13. Of the 6 cases that read no number correctly there was only 1 who claimed to see nothing on any chart beyond Plate No. 1; he appears to qualify as a complete color-blind. Some 5 per cent of those examined reported that they were able to get the digits better at the 10-foot distance than at 4 feet. The improvement was usually not very marked and it commonly applied to Plates 6 to 9.

Summary

1) The Ishihara Test, which consists of numbers 2 inches high printed in colored dots on fields of colored dots, is recommended as a practical test of color-blindness to use in combination with routine physical examinations and by ophthalmologists.

2) This test is very serviceable in selecting cases of color-vision defect for further study.

3) In a group of 1286 university men examined by means of the Ishihara Test during 1927 and 1928, it was discovered that 106 exhibited definite color defect. This percentage, 8.2, is about double that ordinarily found with wool tests for color-blindness.

4) A group of 436 university women showed one clear case and three other faint cases of color-blindness.

5) Among the men green-blindness is more frequent than red-blindness in the ratio of about 3 to 1.

6) In a large number of the cases of color-blindness, the men are quite unaware that such a defect is present. The Ishihara Test constitutes the simplest available demonstration for convincing such cases.

7) A few cases detected by this test show better ability to read the numbers at a distance of 10 feet than at 4 feet, thus indicating greatly restricted color fields, which are practically confined to the fovea.

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THE VEHICLE ILLUSION IN THE VERTICAL AXIS

There are two familiar illusion experiences which for simplicity of description we may set off by the names "vehicle illusion" and "panorama illusion." The vehicle illusion is a very common one. People frequently report and comment on it in reference to the starting of railroad trains in the station. It is always a difficult matter to be sure whether your train or the train parallel to it is the one that has started to pull out. Ordinarily when the motion is slow it is imputed to your own vehicle; you are moving and thus you account for the moving visual field. The panorama illusion appears to depend upon higher rates of speed and the hiding of the foreground or such a distance from the foreground as to make it impossible to see small details clearly. With the train or automobile rushing along smoothly at 50 miles an hour, if one gives his attention to objects 300 yards away at the side, he usually gets the panorama illusion—the objects seem to be moving by him while he is about stationary. The conditions favorable for producing the panorama illusion seem to be ideal in aviation at moderate or high altitudes.

I am not aware that any one has reported the vehicle illusion occurring in the vertical axis. The occasions when we are moved in the vertical by what we may class as vehicles are not as numerous as those providing horizontal movement. I wish to report a very compelling vehicle illusion in the vertical. This occurred at a railroad ferry. The train was broken up into half a dozen sections, which were placed parallel on the ferry boat. The boat moved across the sheet of water two miles or so in width. The adjustments made at a railroad ferry to accommodate for changes in the height of the water due to the tide have to be rather more enormous than those associated with an automobile ferry. The adjustable apron which carries the rails which are to connect with those on the ferry boat is about 100 feet long and 50 feet wide. As our boat made fast to the landing,

the edge of this apron was above the deck level some 10 feet. At first the apron was lowered rapidly, at the rate of about one foot in two seconds. The observer was standing on the platform at the end of one of the coaches. His eyes were approximately 10 feet above the level of the deck of the ferry boat. The impression was perfectly clear that the ferry boat was stationary and that the huge wooden apron was moving down to our level. When the apron reached a point about two feet above its final position, suddenly the vehicle illusion was present in a very compelling form: one experienced the boat gently rising to couple up with the rails on the apron. The movement of the apron at this time was slow and from a position 10 feet above the platform one looked out on the apron obliquely so that this great moving surface covered a large central portion of the visual field. When the rail level matched and the movement ceased, there was an after-image of movement, that is, an after-image of the vehicle illusion just experienced—the deck was sinking slowly. The observer remarked on what he had seen to the train officer who was standing by him, who said, "Yes, it always does that." The tendency, of course, is to watch the last foot or so of the movement quite intently, hence a good many people see the "funny" effect.

Brief experiences of the vehicle illusion in the vertical axis can be got from automobile and passenger elevators. Standing right at the rail of the elevator well in a garage and watching the elevator come up slowly, one gets the impression of motion just as the elevator comes to rest at the proper level. Parallel elevators with rather open lattice cages provide us a situation quite analagous to parallel trains in the station. The chief difference is that with the elevators the motion is more rapid in start and stop, and, as there is only one car rather than a succession of cars, we have much fewer opportunities for being at the same level as the adjoining car, which is, of course, the necessary condition if we are to get the illusion. The speed of elevator movement commonly used introduces a visceral-kinaesthetic factor which serves to cut short the illusion. Due to the force of gravity we are much more cognizant of having a platform drop out from under us or push up against us than we are of having it glide evenly on the horizontal axis. Our usual experience of the vehicle illusion in the horizontal occurs when we are seated, then the body is relaxed and has large areas of support which brace one another.

The vehicle illusion is not limited to the horizontal axis of motion. It can occur in the vertical if conditions are present similar to those that produce it in the horizontal. It appears to be an illusion that is quite independent of axis of motion but wholly dependent upon a certain range of speed, the involvement of a certain amount of visual field, and the absence of certain kinaesthetic factors.

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NEON GLOW LAMPS AND THE DEMONSTRATION OF VISUAL PHENOMENA

The neon lamp is the luminant now commonly employed in the new electric signs. The rapid adoption of this type is due to several special features which it embodies. This new lamp has very low current consumption, a relatively small light output which makes it possible to be viewed directly without discomfort to the eyes, a distinctive color (or colors), and the absence of a fragile filament. These lamps may be made in the forms of thin letters or diagrams which are very legible far away or near to. Usually they are not caused to wink or flicker but are carried continuously on 60-cycle circuits, and, when looked at, the light appears to be steady. For reasons to be mentioned later, it is not necessary to flicker these lights for gaining the attention, as has commonly been done with the regular filament lamps.

A small spherical bulb lamp of this type known as Neon Glow Lamp[1] is now available at a moderate price. The bulb is 1¼ inches in diameter and is mounted on the usual screw base so that it fits the ordinary electric-lamp socket (See Figure 1). No additional resistance unit or other accessory is needed for its operation. By reason of its economical current demand and long life the lamp is admirably adapted to be used for pilot

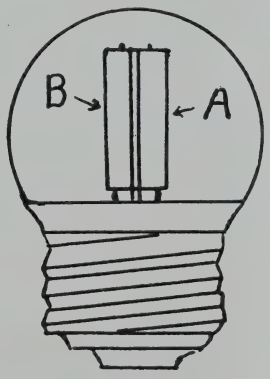

FIGURE 1

DIAGRAM OF THE NEON GLOW LAMP SHOWING THE ARRANGEMENT AND RELATIVE SIZE OF THE ELECTRODES *A* AND *B*

Each electrode is in the form of a half cylinder mounted about a central core of composition material. The electrodes flash alternately.

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[1]Manufactured by Cooper Hewitt Electric Co., Hoboken, N. J., and known as G-10, ¼ watt, 110 volt. Price, about 55 cents each. See (3) for data on this type of lamp.

purposes, serving as an indicator in any place where it is desired to show that current is being used. It is an excellent marker for showing the location of switch or valve in a dark environment. It can be used to illuminate the face of a boudoir clock or placed behind a glass plate bearing the house or street number.

Any or all of these uses apply, of course, to the laboratory, but the most unique features of the lamp for laboratory purposes is that it provides interrupted light (flashes of light) at the rate of the cycle frequency in the alternating current. I am advised by the manufacturer that the "lamp has performed when modulated at the rate of 50,000 per second." It does not come to full brilliancy until it reaches at least 100 volts. Where a readily modulated light source is desired or where a lamp capable of being lighted at high frequencies and with a minimum of lag is needed, this particular lamp deserves trial. For example, it is ideal as a stroboscopic illuminate. When used with the Victor Stroboscopic Disk (2) on a phonograph turntable without other natural or artificial lighting, it gives a very startling result in the sharpness with which the spokes on the disk stand out.

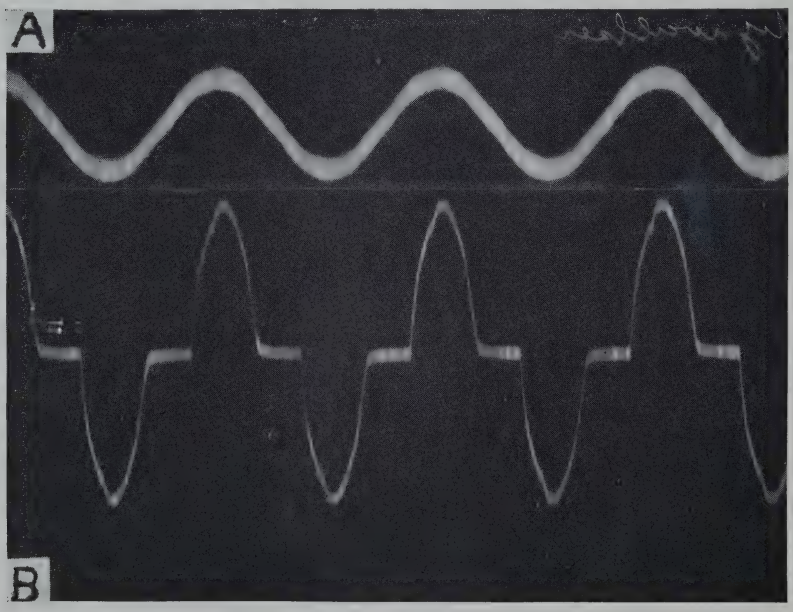

FIGURE 2

OSCILLOGRAPH TRACING SHOWING THE OPERATION OF THE NEON GLOW LAMP

A—the 60-cycle voltage curve. *B*—current alterations in the lamp. Current does not flow continuously but is at zero about 41 per cent of the time.

The element in this lamp is made up of two metal electrodes, each shaped like the half of a cylinder and formed around and covering a central composition core. The electrodes are about 5/8 of an inch long and 3/8 of an inch in diameter. The lamp when operated on alternating current comes to full brilliancy on one electrode, drops to zero on this electrode, then comes to full brilliancy on the second electrode. On direct current of proper voltage one electrode only burns continuously, there are no flashes. These facts for alternating current operation are clearly shown in the oscillograph tracings, Figure 2. At the top we have the 60-cycle voltage curve of the circuit on which the lamp was operated. The lower curve shows the oscillograph record for the current in the glow lamp. Deflections upward indicate the operation of one electrode while the downward deflections were associated with the illumination of the second electrode. From this tracing it is obvious that the current is not flowing continuously in the lamp. The straight horizontal lines made by the oscillograph element record moments of zero current. From zero there is a quick increase in current, much more rapid than the associated rise in voltage in the curve above. There is a rather sharp peak and a swift return to zero. The current does not return to zero as quickly as it leaves the zero line. The zero-current time intervals represent about 41 per cent of the cycle interval or of the phase interval. During zero current the lamp gives practically zero light. The neon lamp thus provides us with an interrupted light which follows the cycle rate on which it is operated. If 60-cycle current is used, each electrode gives 60 flashes per second, and the lamp as a source for lighting some object like a stroboscopic disk would give 120 flashes per second.

If one holds the Neon Glow Lamp 1½ feet or more from the eyes and fixates the electrodes, the light seems to be perfectly steady on 60-cycle current; the element glows evenly as a red-hot iron might glow. If the lamp is moved, the element gives the appearance of quivering, and well-marked lines of light appear on either side of it. If one shifts his fixation from side to side across the lamp, distinct flashes of light appear. It is this latter feature that makes the lamp a good attention-getter. One is visually exploring the road or street, fixating this and that and jerking his eyes back and forth at high speed. Suddenly in the periphery of vision or perhaps reaching right across the fovea one sees a line of transient flashes all about the same size and shape but of decreasing brilliance. The effect is challenging. There is nothing else quite like it. A brilliant twinkling star provides the same effect, but the flashes from this are so faint that most people have never noticed them.[2] The strung-out flashes from the neon

[2]The star flashes are particularly easy to see if one looks at some bright star through a 6-power binocular and at the same time gives the glass an oscillation or circular motion. If the eyes are moved back and forth across a street arc light, we may readily observe whether the lamp is operating on A.C. or D.C.

sign are so large in area and so brilliant that the eyes naturally turn to their source. Here they see outlined in a steady glowing light some word or diagram, and the conditions are most favorable for perception. The interrupted light of these lamps plus the factor of eye movements plus the factor of persistence of retinal impression is the formula which probably provides a better attention-getting and perception-promoting result than came from the earlier flash sign.

The Neon Glow Lamp is an admirable device for demonstrating the phenomena of persistence of vision (modern form of the firebrand experiment) in the lecture room or laboratory. Moving the lamp rapidly back and forth or twirling it through a circle in a dark room, or even in a well-lighted lecture room, provides a visual stimulus of striking quality that can be seen by everyone. Due to the arrangement of the electrodes and to the characteristics inherent in the operation of this lamp as shown in the oscillograph tracing, different kinds of spots can be secured according to the angle from which the eye views the lamp. If the lamp is held so that the observer sees only one electrode and is then moved laterally back and forth, spots of light $\frac{5}{8}$ of an inch high by a width according to the rate of movement will appear. If the movement is rapid, the flashes will be long and the gaps long. The latter have somewhat greater width. The observer is seeing only 60 flashes of light per second, that is, the flashing of one electrode. If the room is dark, a phantom or dimly lighted spot will appear midway between the main flashes. This is due to the illumination of the electrode which is now on the back, away from the observer. The light from this electrode reflects against the glass at the rear of the lamp. This phenomenon of the phantom dashing in between the bright dashes will be seen only against a dark background. Diagrammatically and only approximately, Figure 2 represents the varieties of flashes, for convenience of illustration showing these flashes as dark spots on a white background. The flashes described above are illustrated in *A* of Figure 3. Now if one turns the lamp so as to view the upper end of the two electrodes, i. e., with one electrode above and the other below, as in *B* of Figure 3, lateral movement of the lamp in the direction of the arrow gives flashes as a series of curves, as illustrated. If the lamp is rapidly moved back and forth through an 8-inch arc, one can secure the effect of standing luminous waves that show rather short gaps halfway between the trough and crest of each ascending and descending curve. Slower movement produces crossing or interlacing of the approximate semicircles of light. If the lamp is turned from this position 90° so that the end of one electrode is seen at the left and the other at the right, as seen in *C*, Figure 3, then rapid movement shows pairs of crescents back to back with fairly large gaps between pairs. The inequality in the spacing is caused by the combination of the flashing time of the electrodes and the angular rate of movement of the lamp. If the electrodes are viewed from the side so that each one is seen equally,

D of Figure 3, rapid movement gives pairs of spots each half as wide as shown in *A*, and in arrangement as would be expected from a lateral view of *C* in our figure. With these various possible combinations the flashes are quite interesting objects for study, and if the lamp is mounted on a mechanical device to rotate it or oscillate it, very definite effects can be predicted. Used in this manner it provides us an apparatus for experiments in observing transient visual phenomena.

The Neon Glow Lamp is proving of much service in our Laboratory as a means of demonstrating and making preliminary study of eye movements. As everyone knows, it is difficult to control the visual fixation of an observer. With the very best cooperation, the observer may be unable to report whether his fixation has been unwavering just prior to and at the moment of a certain exposure. The neon lamp is of service in this connection. Placed behind a screen with a very small opening, it can be used as a

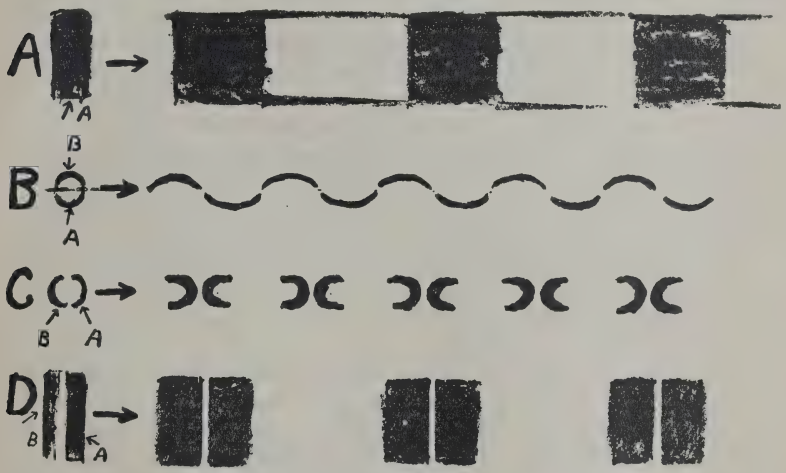

FIGURE 3

PATTERNS OF THE FLASHES SEEN BY THE EYE WHEN THE NEON LAMP IS MOVED RAPIDLY

Small *A* and *B* designate the two electrodes, and the flashes are shown as dark spots.

A—viewing only one electrode. The flashes as diagrammed beyond the arrow are 60 per second. *B*—electrodes viewed on end, one above the other. Movement of the lamp gives the appearance of standing waves. *C*—two electrodes viewed on end, one at the right and one at the left. Movement gives pairs of crescents back to back. *D*—the two electrodes *A* and *B*, both in side view. Movement gives pairs of dots. If the lamp is still and the eye is moved, similar patterns of dots will be seen but they will be farther apart due to the high speed of angular rotation of the eye.

comfortable fixation point. If fixation wavers, the unique phenomenon of the flashes appears. Wavering fixation is thus more likely to be reported than when the fixation point is an ordinary steady light.

In studying the visual phenomena associated with winking the eye or alternately opening and closing the eyes (1), the flashes aid in describing the observations, by reason of the fact that they appear as a new feature which comes in association with eye movement. Subjects can also make an estimate of how many spots of light were seen trailing off and thus state the speed of their eye movements.

For directly demonstrating the high speed characteristics of saccadic eye movements and the interplay of these with pursuit movements and with compensatory movements, I know of nothing that at all approaches the neon lamp in usefulness. With the lamp mounted at a distance and with fixation points placed one on either side and a pair arranged in the vertical representing an equal visual angle, the subject is instructed to look back and forth or up and down between the fixation points giving attention to the flashes and making an effort to count them. From the way the flashes are stretched out he has a definite impression of the speed and notices that this is quite regular for either the horizontal or the vertical movements, but is slower for the vertical. He can make an estimate of the relative speed of different angles of movement by changing the positions of the fixation points. The results are only approximate but much better than can be obtained from observing the eye movements of someone else. In fact they are only second to photographic records. They supersede these in one respect, which is that the subject is also observer and has his results immediately in combination with the experience rather than waiting to see them on the tardily developed photographic plate.

If the subject observes the lamp at a distance, fixates it carefully, and then turns his head slowly, he sees no flashes or flutter of the element. More rapid movements of the head or body cause flashes and prove that his compensatory eye movements are not quite accurate or adequate. If the observer fixates the lamp while it is moved mechanically or in the hand of someone else, and if he holds his head still, his eyes will be executing pursuit movements. If the movement of the lamp is slow and regular, it will appear only as a glowing iron entirely without flashes. When the movement is more rapid or jerky, flashes will appear, proving that the pursuit movements have become inadequate for following the stimulus and that they are being supplemented by saccadic movements. The limiting rate of movement in a stimulus which can be adequately matched by pursuit movements of the eyes can be approximately determined without difficulty. If, in place of moving the lamp, it is mounted in a stationary position and the subject holds a pencil at arm's length in line with the lamp and fixates the pencil, while he moves it slowly back and forth, he will see a great number

of fairly compact flashes from the lamp. In this way he can demonstrate to himself that the pursuit movements are very much slower than the saccadic movements.

Summary

For advertising purposes and as an adjunct for laboratory experiments the neon lamp has the following advantages:

1) It provides flashes of light at a rate governed by the cycle frequency of the current used. It will respond to rates as high as 50,000 per second and is nearly instantaneous in its rise to complete brilliancy and in its decline to zero.

2) Due to the persistence of vision, flashes of light appear when the lamp is moved or when the eyes are moved. These flashes are very obvious and challenging as a new and unexpected feature in the environment, therefore they are quite attention-compelling. It is thus unnecessary to "flicker" the street sign made from this type of lamp.

3) Neon light is not too strong for direct vision. A small electrode of this character may be visually fixated without discomfort to the eyes.

4) Used as a control to insure steady fixation in visual experiments, this lamp aids the observer in reporting his fixation failures.

5) By arranging fixation points around a stationary neon lamp the approximate rate and directness of saccadic eye movements can be observed (counting flashes) by alternating fixation between the points and thus examining horizontal and vertical axes of movement.

6) The adequacy of pursuit movements in reference to the speed of a target (in this case the lamp) can be observed. If the pursuit movement has to be supplemented by a saccadic movement, flashes appear. Similarly, compensatory eye movements associated with head movements and body movements can be studied.

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THE NEON LAMP AS AN AID IN STARTING SYNCHRONOUS MOTORS AND AS A SPEED INDICATOR

Inasmuch as a synchronous motor will run at only a certain speed or a multiple thereof on alternating current of a given frequency, it is necessary that its armature be revolved at this exact speed by outside force before the machine will function as a motor. Because of the difficulty in judging the exact speed at which to turn the armature, it is often a trying task to start this type of motor on laboratory instruments such as the Dunlap Chronoscope, the Lisston Tonoscope, and other apparatus embodying this source of constant speed power. With the aid of a Neon Glow Lamp, such as described in this number by Miles, the task becomes relatively easy.

The minimal speed of any synchronous motor is given by the simple formula:

$$\text{rev. per sec.} = \frac{f}{N}$$

in which f is the frequency of the current supply in cycles per second and N is the number of poles in the magnetic field. Thus, the ordinary speed of a 12-pole motor when supplied by 60-cycle current is five revolutions per second or 300 per minute.

If a circular ring composed of 12 equally spaced black segments, on a white background, is fastened to the rotor or the drive shaft of the motor, each of the 12 segments will pass a given point five times per second. In other words, any one point on the revolving ring would actually be alternately white and black 60 times per second, although it would appear gray due to the rapid mixture of the colors.

If, however, the revolving ring is illuminated by a neon lamp which is supplied by the same current and which is also alternately light and dark 60 times per second, the segmented ring will seem to be standing still. This illusion is, of course, due to the fact that the ring is lighted only while the black segments are in a given position and is dark during the time that it takes them to move to the next position of the adjacent black segment. Because of the very short time interval between the successive appearances of a black segment, positive after-images fill in the gap, and the black appears to be black all of the time.

In starting the motor, it is simply necessary to spin the rotor until the ring appears stationary, at which time the normal speed will be reached and the motor will continue to run on its own power. Such a device may be utilized on any small synchronous motor, since the number of segments needed in the ring is:

$$\text{no. of segments} = \frac{f}{\text{rev. per sec.}}$$

and since

$$\text{rev. per sec.} = \frac{f}{N}$$

it will be seen that

$$\text{no. of segments} = N$$

It will be necessary then to make the same number of black segments on the ring as there are magnetic poles in the motor. As long as both the neon lamp and the motor are operated on the same current supply, the scheme will work equally well with any frequency current. It will readily be seen that the ring would also appear stationary at any multiple of the motor's minimal speed, so the device is convenient and useful in starting the motor at some multiple of its slow speed.

In other instances this same device can be utilized as a speed indicator. Thus, a ring with 12 black segments illuminated by a neon lamp on a 60-cycle current would look to be stationary at 300, 600, 900, 1200, etc., r.p.m. By counting the number of times that the static appearance occurs while the motor is coming up to speed, its approximate speed can be estimated. A slight deviation from one of the above speeds will be indicated by an apparent slow revolution of the disk to the right or left. Using these same principles, it is possible to divide the ring so that it will appear to be stationary at any desired speed. The number of segments necessary is obtained by dividing the desired speed by the frequency of the current available. This makes a very inexpensive and yet an extremely reliable instrument for adjusting a piece of apparatus to a desired speed.

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HIGH-SPEED LAMPS FOR TACHISTOSCOPIC WORK

The development of "high-speed" incandescent lamps provides an important piece of apparatus for use in tachistoscopic and related experiments. Control of exposure by the simple method of switching on and off the current supply of an incandescent lamp has heretofore been seldom resorted to, for the obvious reason that in brief exposures the time taken by the ordinary lamps in heating and cooling is objectionably great in relation to the exposure times themselves. It is plain that in the control of exposure in tachistoscopic work one should have control of the rates of appearance and disappearance of the exposure field; indeed in the interest of simplicity these rates should be kept high in relation to the rates of response of the retina. In short exposures the visual response must be regarded not as a function simply of time, but of the total energy acting on the retina, which is an integration of the light intensity throughout the interval.

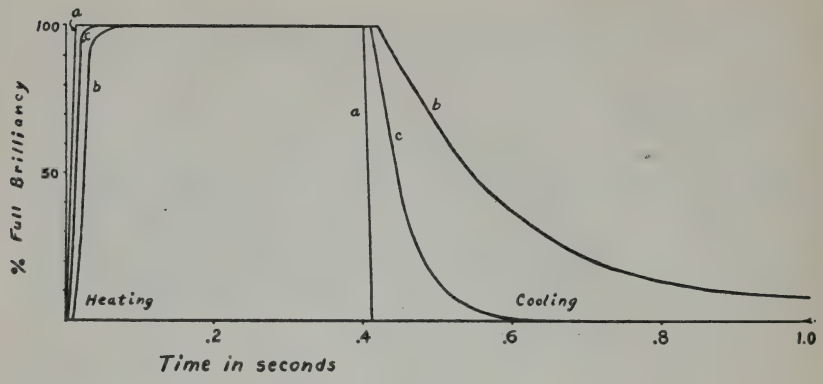

FIGURE 1

a, falling-screen tachistoscope; *b*, 100-watt regular stereopticon lamp; *c*, 150-watt high-speed lamp

The ideal type of exposure would be one in which the illumination of the field was changed instantaneously from minimal to maximal, and conversely; but it is obvious that this ideal condition can only approximately be realized in practice. A close approach can be made by passing before a focussed beam a narrow slit in a very rapidly moving screen. Under best conditions the curve of the illumination of a field opposite the beam will approach a rectangular form when plotted as a function of time, i.e., the curve would be rectangular if plotted in Figure 1. The usual forms of apparatus which utilize this method do not usually, however, obtain curves as desirable as this; they approach more nearly the form of that shown as *a* in Figure 1, which was given by a falling-screen tachistoscope used by the writer (1, p. 200). But, fortunately, because of the inertia properties of the retina just mentioned, one does not need to attain ideal conditions, and an exposure like that shown as *a* in Figure 1 is entirely satisfactory.

The familiar types of incandescent lamp give curves of the kind shown as *b* in Figure 1, and are slow enough in their operation to introduce considerable error into measurements of exposure. Certain lamps of recent development, on the other hand, compare favorably with curve *a*; these are the lamps designated as "high-speed" lamps, one of which is shown in curve *c*, Figure 1. It is plain that this type of lamp recommends itself for use in tachistoscopic work.

Under the designation of "high-speed" lamp several varieties may be included. These lamps have been developed and their properties studied in the research laboratories of the General Electric Company, primarily in response to an expression from the U. S. Signal Corps of the need for a

rapid-acting lamp in war signalling (2). One of the most rapid lamps developed is one which differs from the ordinary lamps in several respects, but chiefly in that the filament is of tungsten drawn particularly fine, immersed in an atmosphere of hydrogen. The use of a ribbon instead of a wire filament gives greater speed, but the vaporization rate of the ribbon is relatively high, and hence the life of the lamp is short. Immersion in argon gives longer life and greater efficiency, but at the expense of an appreciable reduction in speed. It is apparent that the choice of a lamp for a given purpose will depend not merely on its speed characteristics, but will consider also such factors as sturdiness, life, efficiency, and initial cost. For psychological work the wire-filament, hydrogen-filled lamp will probably be the most satisfactory. This lamp may be obtained in several sizes. The writer has had most experience with the 150-watt, 110-volt, G-25 bulb type, which is quite satisfactory in performance and life. The 250-watt, 110-volt, G-30 bulb type is also recommended.

The 150-watt lamp heats up to full brilliancy in 0.022 sec., and cools off to the point where the filament barely glows in 0.280 sec. The corresponding figures for a 100-watt regular stereopticon lamp are 0.068 sec. for heating and 0.786 sec. for cooling. A 400-watt, hydrogen-filled lamp heats up in 0.069 sec., and cools off in 0.834 sec. The 400-watt stereopticon lamp takes 0.196 sec. to heat up, and 3.914 sec. to cool.[1] The advantage of speed is clearly in favor of the hydrogen-filled lamp, by a factor of 3 or 4, for both heating and cooling.

In ordinary tachistoscopic work the effective speeds of the hydrogen-filled lamps are even greater than the above figures might lead one to believe. The reason is that much of the time included in the figures is not significant in respect to a given exposure. A reference to Figure 1 will help to make this point clear. The curves show that a considerable portion of the total time is expended (1) in heating up the filament to the point at which the light emitted is sufficient to illuminate a field, (2) in bringing the heating curve through the upper portion of its course, from about 90% to full brilliancy, which because of Weber's law is not a significant change, (3) in the initial drop of the cooling curve to the point where the diminution of light becomes significant, and (4) in the last stage of the cooling, extending beyond the point at which illumination of the exposure field has practically ceased, and the filament is approaching room temperature. This means that the middle—and fairly straight—portions of the curves are the really significant portions to consider in most exposure work. In other words, the "reduced" times, i.e., the times involved in heating (or cooling) from about 20% to 80 or 90% of full

[1]Data kindly supplied by the Engineering Department of the Edison Lamp Works.

brilliancy, are the significant values for a designation of speed in this connection.

Unfortunately, we do not have sufficient data to say what the "reduced" times will be for the lamps mentioned. Some data are available, however, on similar lamps of lower voltage (2), and a rough calculation shows that for them the "reduced" times of heating are but 25 or 30% of the total times, and for cooling are 20% or even less. Indeed, the effective cooling time is about the same as the heating time. Probably these values will be corresponding for the higher voltage lamps.

It should be noted that the "high-speed" lamps are not made up as a commercial article, but are manufactured on order. Their cost, however, has in the writer's experience been but slightly in advance of the commercial lamps of equivalent rating.

A word of caution should be added regarding the use of a hydrogen-filled lamp. In some instances a leak may develop at the seal, which allows enough oxygen to enter to cause an explosion when the lamp is lighted. For this reason the lamp should be enclosed in a housing of some kind, or covered with a fine wire netting, to afford adequate protection from flying glass. Care should also be taken to provide proper ventilation, so that the heat may be conducted away, or else the lamp will not perform at normal speed, and its life will be reduced. Aside from these precautions the "high-speed" lamp is operated like any other incandescent lamp.

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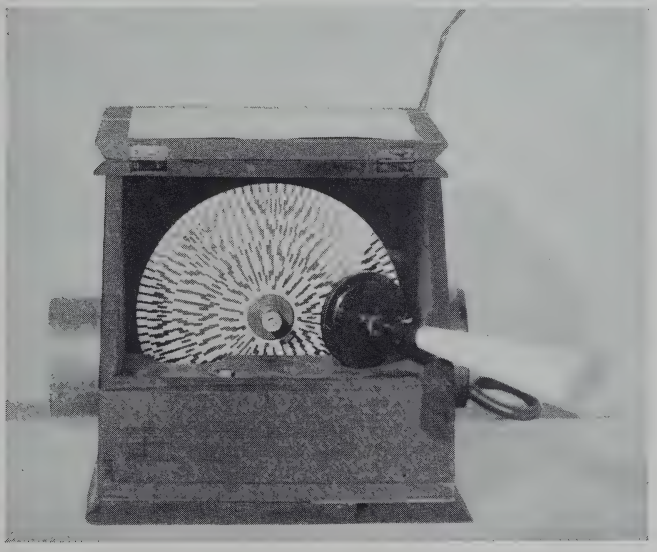
TWO INDEPENDENT DEVELOPMENTS OF THE STROBILION

On the subject of culture, anthropologists have been considering at least two rival theories. One, the Rivers-Elliot-Smith "heliolithic culture" hypothesis, holds that much, if not most, culture has spread directly or indirectly from one source. Extreme proponents of this view hold that discoveries or developments of specific culture items are in general made by a single person, and so rarely more than once. The rival theory states that discoveries, independently made by more than one person, have occurred frequently in different portions of the world. A compromise position between these two views is becoming increasingly popular. It may perhaps be stated as follows: cultures do spread, but are frequently added to in a

similar manner by a number of people. It is the purpose of this note to show how two researchers have built similar structures on the same foundation.

A device which made use of manometric flame and stroboscopic principles for the visual demonstration of vocal sound effects was developed by E. W. Scripture (2), then of Yale. This was altered by C. E. Seashore (5) and C. F. Lorenz (1) and put into more general use by Seashore under the name of "tonoscope" (4).

Scripture has recently constructed a new form of this apparatus, which he terms the "strobilion" (3). From the description and photograph it



seems to be practically identical in its essential details with one developed at the Stanford laboratory. The accompanying photograph is of the Stanford model, which is called the "true tone analyzer."

In the autumn of 1926, Mr. M. A. Lissman, a graduate student in the Department of Electrical Engineering at Stanford, was an auditor in the writer's class in the psychology of music. At this time he became acquainted with the early model of the Seashore tonoscope. During the following winter months he constructed the Stanford model. To the writer's knowledge no mention of this has appeared in the press except for a "doctored" account in a San Francisco newspaper.

The Lissman instrument with motor and gas tank attached weighs but 19 pounds, so is convenient for demonstrational work. The synchronous

motor runs at two speeds—for the optimum display of the voices of the two sexes. A worthwhile improvement over the older instruments is the addition of a wire which is wrapped around the burning point. While the instrument is in operation, the wire becomes hot, and relights the gas whenever too great a voice pressure makes this necessary. While the instrument has not had much use as yet in voice training, it conceivably possesses possibilities in this connection.

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BOOKS

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EPSTEIN, A. L. *Son i Ego Rasstroistvo.* (*Sleep and Its Disturbances.*) Leningrad, Moscow: Gosudarstvennoi Izdatelstvo, 1928. Pp. 408. 3 rbs.

Until recently the pathology of sleep was restricted to insomnia and partly to hypersomnia. The pathology of sleep as the teaching about disturbances of a precise function with all their typical clinical characteristics did not yet exist—hence the deficiency of theoretical knowledge concerning the physiology, mechanism, and structure of sleep. However, the data of the last epidemic of lethargic encephalitis led to the finding of those physiological and pathological grounds which made it possible to unite various sleep disturbances in a harmonious organized system of sleep pathology. At the same time there was also elucidated the question concerning the mechanisms of normal physiological sleep, as of a function complicatedly coordinated and complicatedly centralized in the brain. This book by Dr. Epstein aims to give a systematic exposition of present day theories of sleep and of its disturbances. The fact that the author pays much attention to the mechanisms of normal sleep makes the book of great interest from the standpoint of general psychology.

In the first chapter the author gives a general conception of the anatomico-physiological basis of normal sleep, according to different data of the literature on the question. An attempt is made to establish a correlation between the anatomical data and the West European physiological theories of subcortical mechanisms of sleep, on the one hand, and Pavlov's teaching about the cortical mechanisms of sleep, on the other hand. After this anatomico-physiological introduction, an exposition of somatic and psychical alterations during sleep follows (Chap. 2). This chapter is based not only upon the data of the literature but also upon the personal investigations of the author. Out of the somatic modifications, the author annotates, in the first place, the relatively small group of visceral altera-

tions; this done, he gives a detailed description of neuro-muscular alterations. The analysis of psychical modifications during sleep (the most detailed description is given for the cases of "hypnic attention," "hypnagogic hallucinations," and of "ascending psychomotorness") is of special interest because the author tries to correlate these modifications with the clinical symptoms of the disease (autism, loss of the feeling of reality, hallucinations, double-personality, physical influence, delusion, etc.).

Chapter 3 offers a classification and a general semeiotics of sleep disturbances. The next six chapters contain a description of separate groups of these disturbances: casual, secondary, constitutionally neuropathic, reactive, symptomatic, and essential. (The greatest attention is paid to the third group.) In Chapter 10 ("Sleep and Psychoses") the author gives a historical analysis of different conceptions of the correlation of sleep and psychoses, and states the following three stages: of analogies, of clinical observations, and of pathophysiological and pathopsychological analysis. In Chapter 11 we come upon the pathological states related to sleep. Finally, in the last chapter the author speaks about sleep disturbances from the standpoint of forensic medicine. The book is written very concisely, almost in the form of a conspectus, and offers a summary of all present-day theories of mechanisms of sleep and of sleep pathology; its appearance is, therefore, of great value.

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Gaskell, A. *What Is Life?* (Introductions by K. T. Compton and R. Pearl.) Springfield, Ill.: Thomas, 1928. Pp. 324.

One view of science, regarding science for the moment as merely a more or less agreeable means of passing the time, holds that the business of science is transacted through the discussion of theories. Unfortunately, this conception is half correct. But everyone is familiar with those amusing states of mind (usually evidenced by other people) in which the wonders of nature assume significance solely in relation to "my" theory. Hence the danger of taking theories too seriously; one's personal theory especially—because it may turn about and bite one. This general notion as to what science is about sometimes results, moreover, in the tacit assumption that the building of theories is exclusively a literary performance. Most books about biological theory have been of this character. It is relatively easy to speculate in print about animals and plants, because (so far as concerns a number of matters of prime importance) it may be difficult for anyone really to prove that you are wrong. And in this case it becomes the duty (?) of science to prove whether one is right.

A good many years ago it was suggested that the substance of living cells constitutes a special kind of molecule—perhaps all the substance of one cell might be such a molecule. In spite of the large molecular weight now assignable to some proteins as they exist, for example, in solution in a blood, there is good reason to distrust the "giant molecule" conception of protoplasm. The chemical events occurring in it are altogether too much like those familiar in non-living systems. Little or nothing has been gained, save obfuscation, by regarding protoplasm as ineluctably "colloidal," with the implication that its phenomena do not make sense in terms of classical physical chemistry, because "colloidal" must itself be so explained. At the opposite extreme, it has appealed to a number of writers, notably physicists and physical chemists, that perhaps the peculiar cussedness of protoplasm, regarded as an engine, might lie in its escape from the restrictions of the second law of thermodynamics. The rules according to which the thermodynamics game is played are derived from observational acquaintance with matter in bulk. If protoplasm is essentially a series of minute reaction chambers, each or many of them perhaps including but a few molecules (of some particular sort), then the statistics of energy distribution might permit, with small enough numbers, very special and peculiar deviations from the ordinary expectation of entropy increase. This point has been made repeatedly, and apparently independently, by such men as Gouy, Freundlich, Donnan, Lewis, and most recently by Page. Like most such things, it is doubtless contained also in Gibbs's celebrated memoir. But the trouble is that until someone demonstrates that organisms *do* evade the Second Law, one need not get excited about it. Where the chemical activities of living matter are most directly open to inspection, as in the metabolism of the chemo-synthetic bacteria or in muscle, we find no reason to postulate rules other than those adequate for non-living systems.

Again, it was at one time suggested that enzymes represented the stuff of life, the essential physical substratum of liveness. This was safe enough, because no one knew just what enzymes might be. When chemical analyses of protoplasm were first made in a comprehensive way, no one substance could be picked out as the "live" component of protoplasm; but the enzymes had not at that time been recognized as separable constituents of protoplasm, and this difficulty was not overcome until Buchner's work with zymase. With increasing work devoted to the mechanics of enzymatic catalysis this sort of mystification has lost some of its punch, although in a different, chemical sense the enzymes of the hydrolytic type do in a way bridge the gap between the living and the not-living.

Throughout all this development there has run the attempt to find, in one way or another, *a* form of matter which might be held responsible for livingness. Its most recent aspect has been the thought of recognizing in the gene of the mendelian analysis a "unit of colloidal living substance."

There is reason, however, on which to regard the exposition of this view as perhaps deficient, since there arise logical problems associated with the notion of a gene which seem to interfere with speaking of a gene as a substance. The conception of proto-protoplasm as *engine,* rather than as *"a* substance," still awaits development. The essence of this idea is that livingness depends on structural arrangement of parts rather than on the presence of matter of a unique type.

The author of the book under review courageously attacks the question, "What is Life?" by way of the substance concept, but with a new approach, one verbally at least more in tune with the newer physics.

Assuming a condition of critical concentration of ions, and the (modified) Bohr atom, the author states (pp. 113-157) a theory of affairs in living matter, which I venture to condense to the following.

There is postulated for the origin of life a local, momentary condition of ionization in which the crowding of ions is such that some atomic domains (outermost electronic orbits) overlap. This is assumed to acquire critical attributes when there arises proximity of some atom or ion which will readily part with an electron to hydrogen ions, preferably to a H_2^+ ion (positively charged hydrogen molecules having two nuclei and one negative electron are known to be capable of a short existence). When the negative electron of the negative atom or ion gets into the field of the positive hydrogen nucleus of the H_2^+ ion with *its orbital* electron at aphelion, *"something is bound to happen"* (italics of the original); one possibility is that the onrushing negative orbital electron will directly collide with the positive electron and will form an exceedingly close union with it. Such a union would be unlike a hydrogen atom, because a unit of the postulated sort would not have the orbital electron revolving at a distance from the positive electron. It could not form [at least the usual] chemical combinations. And it could not become a "charge" on another atom. But it may itself capture electrons and build up a new structure (unless it be torn apart by the fields of other atoms), with electromagnetic properties peculiar to itself. (One is reminded that of the lighter elements, making up the greater part of protoplasm, the possibility exists that isotopic segregations have occurred. The single attempt known to the writer to have been made to test this, with reference to chlorine, gave a negative answer.)

From this hypothetical (but, according to Professor Compton, not impossible) structure, there is deduced the existence of a "dual system"—one part material, the *Y*-system, since built of atoms; the other immaterial, the *Z*-system, since "not patterned after the manner of the chemical elements." The dual system describes certain properties of organisms; the *Z*-system accounts for the living state and answers to life—*"is* the life of the organism."

On this basis, life is an immaterial quantity; death is the separation of

Y from *Z*. From the peculiar manner of combination of the *Y* and *Z* components, the psychic properties of life emerge.

In just what way such a scheme really differs from, for example, the nomological system of Mach, which happened to employ different letters, it is hard to see. Mystical notions are not improved, fundamentally, by draping them in physical terminology. To speak of a type of electron plus proton combination hitherto unrecognized as "immaterial" because it does not fall into the types hitherto recognized in the category of matter, is a rather weak sort of trick. Its importance in this book is due to the fact that having once used the word "immaterial," the author associates with it all the "non-material" attributes of organisms about which she has read. Since life is defined (*Z*) as a quantity, "it follows that the quantity 'life' is identical with 'soul,' and all problems of the soul and of mind are problems of the quantity 'life.' "

This theory, developed as an exercise in logic, is preceded by a discussion (pp. 1-112) of the nature of theories in general, of organisms, and of matter and the atom. It is followed by an examination of the elements of originality contained in the theory. Among these (pp. 169 *et seq.*) are several concerned with psychology. The method of approach is advanced as a new, exact, quantitative method, to be based upon such assertions of the theory (though not, as the reviewer sees it, necessarily derived from it; at least the derivation is not made clear) as the following.

Mind is the electrical organ (*Z*-system) within the brain. The method of investigating it will have to be the exact methods of the physicist and mathematician. We are not told how they are to be applied. However, it is pointed out, for example, that the superior psychic powers of adult men indicate that the human brain carries a far greater and more complex *Z*-system than the ape's brain carries, the former being acquired during prolonged infancy. On this basis, the duration of infancy, it is proposed to measure the comparative intelligence of races. How infancy is to be measured is not indicated.

There is also given (inevitably) a theory of the cause of cancer and a theory of heredity and a consideration of the origin of species, also a glossary of scientific terms, which is well done.

The proof of this notion is to be had only by physical examination of living matter. The conditions of abiogenesis, the construction, that is, of conditions under which *Z*-systems might form, are not described. The emission of radiation on death might be looked for, but well-known investigations show that "living" and dead albumin do not detectably differ in energy content; Lepeschkin's recent work is of dubious significance in this connection. What use the author might have made of Gurwitsch and his "mitogenetic rays" one is curious to learn. As Pearl points out in his introduction to the volume, the speculation is original and ingenious, though

the biological deductions given as derived from it are unquestionably premature, based upon a wide and varied reading not always critically sifted, so that the argument is sometimes touchingly verbal. It seems to be felt that its central theme may help to overcome what Pearl speaks of as the "quasi-religious inhibition of efforts at investigation of the transition zone between non-living and living matter"; that there is such inhibition one is permitted to doubt. Experimental abiogenesis can scarcely be undertaken without knowing more than we appear to know at present about the essential features of what one would set out to create. That the extension of physics and chemistry to the realm of the living will result in the amplification of conceptions of "matter" seems reasonable; that Mrs. Gaskell's mode of association of protons and electrons may represent the sort of thing which can occur is perhaps valid; but at present the reasoning which this book holds out to support such a conception seems vague and somewhat unnecessary.

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Mason, F. [Ed.] *Creation by Evolution.* New York: Macmillan, 1928. Pp. xxii+392. $5.00.

An admirable attempt has been made by an intelligent editor to gather into one volume a plain statement of those aspects of the evolution concept which seem susceptible to simple treatment. The collection is introduced by Sherrington, following a somewhat unnecessary foreword by Henry Fairfield Osborn. An air of authority is given by the historical statements under the names of the contributors in the table of contents, which indicate the present academic status, connections, and achievements of the writers of the several chapters. (Curiously enough, there is a misprint in the title of the chapter due to MacBride.) In orderly succession the presentation ranges in 24 steps from considerations of the nature and meaning of "evolution" (Jordan, Thompson); its reality as a process (Jennings); the types of evidence requiring its recognition (Parker, MacBride, Conklin, Scott, Bather, Gregory); the evolutionary panorama (Woodward) of plants (Gager, Berry), butterflies (Poulton), bees (Shipley), ants (Wheeler), horses and elephants (Loomis), birds (Watson); the lineage of man (Lull, Gregory, Holmes, Smith); the inevitable notion of progress (Huxley); the problem of mind (Morgan); and a schoolmasterly summary of the evidence for evolution (Newman). The chapters close with a list of recommended books; the editor has supplied as tailpieces a variety of suggestive quotations from diverse sources; and there is a useful index.

Probably, if the truth were told, biologists are somewhat bored with

evolution. As a conception, it is necessary; as a means of codifying known facts about organisms as an assemblage, it is convenient; it reconciles what has been learned about the diversity of organisms. As to "proving" it, this is scarcely in question. To borrow a phrase from J. W. N. Sullivan, our objection to alternative views is not that they are "wrong," but that "they look so funny." The phrase "experimental evolution" once had power, but organic change of heritable character induced by experimental treatment is not very much to the point. One cannot "experiment" with 100,000 years. So that adherence to the doctrine of evolution becomes a matter of acceptance by consent rather than of "experimental proof." When one considers the boldness of speculation formerly rife as to the "causes of evolution," one is at least struck with admiration for the youthful vigor of biology which permitted or even required such a froth, because sober reflection might well suggest that, until something more has been learned about the actual nature of living matter, such speculation may be an agreeable means of passing the time or of cultivating rhetoric but can be fruitful only in some dubiously accidental way.

One notices with gratitude that in the main the contributions to this volume have been content to describe phenomena, within their direct experience, which require for their rational comprehension a relationship of organisms through community of descent with progressive change. Since the book is designed to combat unintelligent, emotional antagonism to evolution, this is obviously the most decent and probably the most effective method. These are not then so much sermons about evolution as recitals of fascinating things seen.

A reservation must perhaps be made in connection with the treacherous term "progress." Julian Huxley considers that the course of adaptation, as made out paleontologically and by the inspection of living forms, requires that we speak of *biological advance* as real *biological progress*. It is not clear that the notions of temporal *advance* and of unfolding complexity have here been sufficiently disassociated. One related difficulty is thought to be disposed of by Huxley's view that the coincidence of his "biological progress" with much of our own common conception of progress and value is due to the fact that man happens to be himself in the main stream of biological progress, or at least likes to think so. Doubt may well be raised as to the extent to which this solution of the problem of teleology can be satisfying or is just superficially clever in a verbal way. Huxley conceives that perhaps "the forces of evolution conspire to act as 'a power, not ourselves, which makes for righteousness.'" Heredity, incidentally, is here referred to as a "force," which is neither illuminating nor correct.

As Gregory puts it in this volume, the knowledge that man has "struggled upwards to his present estate from less intelligent animals is still practically denied to the majority of mankind." Biologists have not infrequently

sought to oppose smothering dogma by preaching the "gospel of evolution." Of course, it is not so important after all that the majority of mankind is uninstructed in biology, but the fact that they may be dogmatically denied the privilege is very important indeed. The tone of the present book is in this respect good, and its effect should be lasting. The chapter dealing with apes (S. J. Holmes), though in places "soft" as to interpretation, tells an interesting story of their human, not to say humane, side. The section by G. Eliot Smith tells us in general terms, and with freedom from controversy, the results obtained from the comparative investigations of localization within the brain, and of the closeness of the similarity of that of man and of chimpanzee and gorilla. He concludes that both brain *and* mind of man are the results of evolutional development. Detailed evidence for this, in terms of behavior, is of course not provided.

A delightfully written chapter is that by C. Lloyd Morgan on "Mind in Evolution." He sees in all cases to which the word evolution can be applied the general fact of *passage,* according to a plan which can be formulated (after the event), "things" being clusters of events in passage. Events pass, but a *state* of flowing events may be persistent, as with social organization or with mind. Evolution he would use in a restricted sense to denote progressive upward change, implying the directive immanence of God; dissolution, assumptions of lowered status, the breaking-up of wholes or emergents, are to be distinguished from evolution—though perhaps necessary for it. The distinguishing feature of mind he finds in *enjoyment* (coupled with its obverse), *reference,* and *guidance* of conduct. In accordance with the level of evolution attained, these three attributes conspire in a degree which determines the evolutional level of mind. This reduces to the problem of discussing the antics of an amoeba in terms of enjoyment and reference, since Morgan is undecided as to evidence of *guidance* of behavior in such forms. The reviewer is frankly skeptical of the verbal worth of this method of resuscitating the pleasure-pain hypotheses; he is quite certain of its analytical worthlessness. Evolution of mind is then to be considered in terms of the extent to which prospective guidance of conduct can be recognized in the animal series, and thus freedom of choice. We are then left with the thought that this must have come in somewhere.

One arises from the perusal of this quite well-made book with renewed conviction that, however necessary as defensive or charitable pedagogy, such discussions are outside the main stream of biological development. That fossil remnants can be arranged to provide suggestive seriations of organic forms is no proof that these series are *the* routes of descent. What does emerge is a large-scale view of the world of organisms, in which the composition and pace of the mass movement are of evolutionary type and dimensions. That was settled long ago. Interest now focusses

upon the factual relations evident within organic systems and upon the properties of living matter as these may be learned about from the quantitative study of the activities and in the broad sense the behavior of organisms. Fifty years from now it may be possible to return to the evolution problem, in the light of what may be learned by then, and to discuss it with less verbalism than is possible at present. In the meanwhile, clear presentations of the views of working biologists, as given in this book, should do much to preserve for investigators a measure of safety in freedom, even in a democracy.

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Fry, T. C. *Probability and Its Engineering Uses.* New York: Van Nostrand, 1928. Pp. xiv+423.

There is increasingly widespread realization that statistical methodology is not after all so terrifying, and is becoming more necessary—at least in the sense that others are using it. Treatises on statistical method are usually either mere handbooks of formulae, of undiscussed history but with presumably mysterious powers; or else, likely to be so abstruse or obscure as to be difficult to digest. Fry's *Probability* is neither of these. It is an admirable treatise on probability, written with originality, verve, and even with a sense of humor. It is not exactly easy reading, but even a non-mathematician, such as the reviewer, is able to turn to it for guidance and comfort. The presentations are clear and orderly, and the practical uses to which the procedures advocated have been put in the working life of the Bell Telephone Laboratories give them a reality not always visible in more academic discourses on chance and reliability. A chapter on the nature, measurement, and units of "probability" establishes concrete conceptions of the term. This is followed by a discussion of permutations and combinations, including factorials, gamma function, binomial expansions. Chapter III deals with various aspects of the theory of probability and its classifiable types. Throughout, numerous examples are discussed (including the problem of card-guessing by a medium) and others are provided for working, while clever use is made of the classical paradoxes of the theory of probability. Chapters IV and V deal with the relations between probability and experiment, and the distinctions logically required by common sense are given illuminating use. Probability is not an experimental science, but experimental test none the less gives a practical basis for *approximate* evaluation of probabilities. The consequences of this view are carefully developed with regard to probability conceived as a limit. Bernoulli's theorem, Stirling's formula, and Bayes's theorem give

basis for a very suggestive discussion of the problem of sampling and experiment. Bayes's theorem is accepted as sound, *"when we can get a grip on it.* The trouble is that we so seldom can" (p. 128). It is regarded as a clue to interpreting the influence of new knowledge upon probability.

Distribution functions and continuous variables are treated in Chapter VI, averages (including expectations of a probability) in Chapter VII. Chapter VIII contains careful illustrations of the use of distribution functions met with in engineering practice. Detailed apparatus for the use of Poisson series is provided; this is followed by an exposition of the normal law, of Pearson's curves, and of Gram-Charlier series. The relations between these are clearly presented. As to the normal law, "though it can do us no harm to know how far men have been able to go in putting a foundation under an ancient monument, we shall be wise not to overlook the fact that it is, in a sense at least, venerable principally for its age. It has its uses, but it is not divinely ordained for the cure of all statistical woes. Other distribution functions which have to do with continuously varying quantities are in no better logical standing." An indication of the suggestive quality of the book is given by the section closing this chapter, in which it is pointed out that by transformation of a variable any distribution of (x) may be expressed as the distribution of some function y (x), such that y may be chosen to conform to any law we may desire. The process might be tried of treating statistical data by finding what transformation would result in a completely random distribution (i.e., equal probabilities for all intervals within a certain range).

Curve-fitting, the measure of goodness of fit, the determination of distribution functions, Sheppard's corrections, are discussed in Chapter IX. This completes the treatment of probability theory.

The primary problem of curve-fitting is taken to be that of determining how unlikely, and by how much, results from experiment are to differ from true probability, and thus of finding the relation between these two. What is needed is some method of judging the accuracy of an estimate without knowing its *inherent* probability before the experiment was performed. With the necessary information at hand, Bayes's theorem gives this. This depends not only on the experimental evidence but on all the other relevant knowledge which goes into the advance judgment. "It is a matter of cold fact that the reliance which we dare place in the result of an experiment depends upon the inherent plausibility of the result as well as upon the accuracy with which it has been obtained." A knowledge of the a priori probability is *necessary.*

Of the sections of this general field usually required in biological and psychological work—measurement of dispersion, curve-fitting, tests for goodness of fit, and correlation—all but the last mentioned are fully and delightfully discussed with a direct, consecutive quality, and with a reality

due to contact with actual data, such as is refreshing. Without discussing "statistical methods" at length, Fry gives, however, a picture of what one may expect to learn from statistical analysis, and its logical limitations, with directions to sources for details and explanations of standard terms. The brief discussion of control charts is suggestive in several ways. Chapter X devotes 65 pages to the applications of the theory of probability to detailed problems of congestion in telephone service, which indeed can supply models for other and quite different situations; Chapter XI deals with phenomena of fluctuation in physics.

Appendices provide tables of factorials, normal curve and its derivatives, the error function, Poisson series, *P*, and the like. The index might be fuller. Throughout the text, at chapter ends, adequate references are given to fundamental books and papers.

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Rabaud, E. *How Animals Find Their Way About: A Study of Distant Orientation and Place-Recognition.* (Trans. by I. H. Myers.) London: Kegan Paul; New York: Harcourt, Brace, 1928. Pp. ix+142. $2.75.

Although not so stated in this volume, this book is a more or less verbatim translation of the monograph published in 1927 at Paris (Alcan) under the title *L'Orientation Lointaine et la Reconnaissance des Lieux;* the essential ideas here involved are perhaps not to be translated with exactness as "distant orientation and place-recognition." The author is chiefly known for his treatment of large biological themes with frequent references to his specific knowledge of insects and arachnids. In other works he has discussed extensively and in rapid succession such topics as heredity and variation, reflex immobilization, adaptation and evolution, convergence, and the like, and his large treatise *Elements de Biologie Générale* is noteworthy for a certain breadth of view coupled with a nicety of statement and a desire to deal in an illuminating way with factual details—in this respect measurably distinguished from the theorizings of such French writers as Le Dantec and Bohn.

His problem in the present book has to do with the systematic experimental analysis of the orientation of progression in animals, and their exploration and examination of new and old scenes. The author very properly remarks that most of the experimentally obtained information relates to invertebrates and more especially to insects, while rigorous and well-conceived experiments with vertebrates are still for the future. Even so, one is impressed by the lack of precise experimentation as concerns the invertebrates. He deals with the usual theories of *orientation* as involved

in the *migration* of birds and considers that these two phenomena are to be kept distinct; it is not entirely clear just how this is to be done nor what is the accruing benefit to understanding. All phenomena of distance-orientation and place-recognition he believes to give evidence of sensory memory. In different cases among invertebrates diverse sensory cues appear to be significant, but Rabaud does not sympathize with those who would appeal to "unknown senses" or rely upon the hypnotic effect of pseudo-physical analogies.

One is compelled to recognize, however, that this healthy attitude is largely a declaration of faith, for, as the author says, the field is free for research if preconceived "solutions" are abandoned. This implies the desirability of quantitative experimentation, which in connection with "homing" behavior has been singularly lacking. The recent excellent work of Wolf, notable for its quantitative form and spirit, is not reviewed by Rabaud, nor is all of the older literature analyzed. The more important evidence concerning orientation of ants, due to Santschi, Cornetz, Brun, is reviewed in an interesting way, with the conclusion that in nature orientation is controlled by a complex of a number of forms of stimulation. The possibility of precise interpretation by means of experiments in which isolated modes of response are first studied separately and then in combination has not been explored.

It may to some readers be a relief to follow a treatise on *orientation* in which tropisms are not mentioned, nor Loeb's comment on wasps; but anyone with first-hand knowledge of what can be learned about tropistic behavior in ants, for example, can afford to smile. As it is, Rabaud is left with the notion that with ants, as with flying Hymenoptera, it seems that the ant "registers" its usual environment as a whole, but in such a manner that it is able on a return journey to neglect zig-zag turnings and to pursue a shortened path—a "matter of kinaesthesis and appreciation of distance"; none of which is especially illuminating.

The recorded information regarding other invertebrates is treated in a fragmentary way. The reviewer may be pardoned for pointing out that in his reference to the behavior of *Chiton,* described by the writer, Rabaud has missed the point completely, and has moreover confusedly attached some of the facts made known in a series of papers (not cited by Rabaud) on *Onchidium* to the label *Chiton,* so that the whole matter is needlessly muddled. There are other instances of hasty bolting of notes on forms outside the arthropods.

The bibliography is confessedly short; the index is a useful addition to the French version.

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